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Geoscientific Model Development Discussions

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Interactive comment on "Implementation and assessment of a carbonate system model (Eco3M-CarbOxv1.1) in a highly-dynamic Mediterranean coastal site (Bay of Marseille, France)" *by* Katixa Lajaunie-Salla et al.

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1 General comments

1.1 Appreciation of the manuscript

In this paper, K. Lajaunie-Salla and co-authors present Eco3M-CarbOx, a biogeochemical model of the carbonate system and the plankton food web. It is integrated into

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the *Ecological, Mechanistic and Modular Modelling* framework, Eco3M (Baklouti et al., 2006b). In the paper, it is applied to the Bay of Marseille (Mediterranean Sea) for which there are comprehensive data time series available.

I found the study interesting, but the paper has, unfortunately, a number of weaknesses. The paper is well-readable, but there remain numerous English errors.

This manuscript has been submitted as a "model description paper." The model description in the main text reduces to thirty-two lines of text only (plus a schematic and a few equations in the appendix), of which fifteen deal with the new carbonate system module. These fifteen lines contain only a few commonplace statements followed by a sequence of eleven reference citations. This is far from what I expect to read in a "model description paper" in *Geoscientific Model Development*. It is also far from what is expected for that type of publication (see http://www.geoscientific-model-development.net/about/manuscript_types.html#item1). Accordingly, I do not even think that this manuscript fits the scope of the journal in its current form. Details about the approximations, numerical methods adopted and algorithms used, their applicability and their limitations are completely missing.

Furthermore, some of the model experiments also have critical shortcomings: it is not realistic to assume that river water intrusions only impact the Total Alkalinity, TA, budget but not that of Dissolved Inorganic Carbon, DIC (riverine TA is mainly carried as HCO_3^- which impacts the DIC and the TA budgets alike).

For upwelling events, only a temperature effect is considered. However, upwelling events also bring nutrients, DIC and TA to the surface. The effects of these latter are not considered in the paper, possible effects not even discussed.

I think the authors will first have to make up their mind whether they want to consider their manuscript as a "model description paper" in *Geoscientific Model Development*, or whether they would prefer to focus on the data analysis and interpretation. In both cases, they will have to extend the model description and revise the experimental design; in the second case, it would be recommended to submit this paper in other journal, such as, e.g., the sister journal *Biogeosciences*.

I am nevertheless convinced that this paper could make a valuable contribution to *Geoscientific Model Development*: from what I have been able to grasp from the paper and the code, the model approach looks solid and it could certainly be applied to other regions of the World as well. I therefore encourage the authors to go for the first option and prepare a **major revision** of their manuscript that includes a comprehensive model description and a sound experimental design.

2 Specific comments

2.1 Abstract

The abstract is not well focused and the hesitation between a model description and a data analysis approach is strongly visible.

2.2 Model description

The model description is completely insufficient. For the underlying model, upon which Eco3M-CarbOx is built, only the ecological structure is summarized. Nothing is said about the spatial extension adopted: is it a point model? does it have some spatial extensions? 1D, 2D, 3D? In the code, one can see that state variable arrays are three-dimensional, but it is not clear if the three spatial dimensions are actually used: the applied pressure, e.g., is set to 1 bar throughout, as if the model was applied for a water depth of about 10 m only. If the model has some spatial extension, how are the lateral and bottom boundaries treated? The physical processes, although mentioned from time to time, are not at all dealt with here. How are they (e.g., transport processes) represented? This lack of description is rather incomprehensible as the authors themselves emphasize in their description of the study area that the biogeochemistry in the Bay of Marseille is “highly driven by hydrodynamics” (p. 3, l. 99).

Any carbonate system speciation calculation procedure rests upon a TA approximation and a pH solver. Here, we do not read anything about these two elements:

- What TA approximation is adopted, i.e., which acid-base systems are taken into account?
- What pH-scale is adopted?
- Which numerical method is used to solve the resulting pH equation? What are the limitations of the adopted method (some methods fail to converge for low salinity water samples, e.g.)?
- Which parametrisations have been used for the stoichiometric constants?

I have been able to find answers to some of these questions by browsing though the code (although I am still not sure which pH scale is actually used in the end—probably pH_{tot}). These informations must nevertheless be given in the main paper. It should not be necessary to inspect the code to find such basic informations.

As the model description stands, there is no way to reproduce the model results, a main requirement of model descriptions in *Geoscientific Model Development*.

2.3 Experimental design

As mentioned above, I find that there are inconsistencies in the design of the model experiments.

In the model upwelling events, only a temperature effect is taken into account. However, as stated on p. 3, l. 100, such events also bring nutrient rich waters to the surface. Accordingly, they should also perturb the nitrate, DIC and TA balances. This is not what the model results reflect: they witness of cooling events only.

Similarly, only part of the effects of Rhône river plume intrusions on the carbonate system are taken into account: the experiments only consider the resulting TA perturbation, but not the DIC perturbation. To my best knowledge, rivers mainly carry TA in the form of HCO_3^- which impacts the DIC balance as strongly as the TA budget. I am even wondering—but could not find any decent data—if the River Rhône water does not also have high pCO$_2$, in which case it would even carry more DIC than TA.

2.4 Code

The code is provided on Zenodo and is easy to download. No reference to this manuscript is given on the model's entry page on Zenodo though. I have not tried to compile the code but only browsed through it as I was interested in getting at least some basic information about the new carbonate system model announced in the title.

The code is commented, but most of the comments are unfortunately in French. This is especially annoying for the `Makefile` and the initialisation file `BIO/config.ini` which cannot be understood without a good proficiency in French.

No user manual is provided, neither on the Zenodo page nor as a Supplement to the paper.

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3 Technical comments

Throughout the paper: please always specify which pH scale is used for reporting the data and model results (in tables, on graphs, etc.)

p. 1, l. 5: is co-author "Irène Remy-Xueref" not actually "Irène Xueref-Remy"? (in the reference section at least the name is spelled that way and that is also the name registered in the submission system)

p. 1, l. 18: "22 states variables" should read "22 state variables"

p. 1, l. 33: "2018, May to 2019, May" should read "May 2018 to May 2019"

p. 2, ll. 42–43: this is unclear. I would not range the biological pump among the physical ones. But it is not sure what is meant here by "physical" pumps.

p. 2, l. 54: "dynamic" should read "dynamics"

p. 2, l. 54: Why only "amplify"? A priori, the forcings could just as well attenuate or reduce acidification.

p. 2, l. 56: "At a global scale" should read "At the global scale"

p. 2, l. 58: "as a net sink or source" may be more appropriate.

p. 2, l. 63: 'MacKenzie" should read "Mackenzie" (also misspelled in the bibliography)

p. 2, ll. 75–76: “strong winds events” should read “strong wind events”

p. 3, l. 86: “implemented within” should read “implemented into”

p. 3, l. 95: Please delete “inhabitants” (“a population of ca. 1 million” is sufficient).

p. 3, l. 97: “winds conditions” should read “wind conditions”

p. 3, l. 101: “intrudes in the BoM” should read “intrudes the BoM” or “intrudes into the BoM”

p. 3, ll. 103–104: “diverse anthropogenic forcing” should read “diverse anthropogenic forcings”

p. 3, l. 106: delete “city”

p. 3, l. 113: “modeling platform” should read “platform” (there is no need to repeat the “modeling”)

p. 3, l. 113: The paper cited (Baklouti et al., 2006a) is not adequate, as far as I can see. The companion paper Baklouti et al. (2006b), which describes the platform would be more appropriate. Baklouti et al. (2006a) review mechanistic formulations for key processes that control phytoplankton dynamics and present a generic model, less so the platform. Please check this.

p. 4, ll. 129–134: The TA production and consumption rates are stated in a very imprecise way here. As such these statement do not make much sense. It should be specified what are the references for the stated TA changes (e.g. TA decreases by two moles *for each mole of* $CaCO_3$ precipitated, and by x moles *for each mole of* XY assimilated by phytoplankton, etc.)

p. 4, l. 132: “when bacteria mineralized” should read “when bacteria mineralize”

p. 4, l. 147: please delete “However” which does not make sense here.

p. 4, l. 148: “model results” can be compared with the observations, not the model itself.

p. 5, l. 161: “winds specific conditions” should read “wind-specific conditions”

p. 6, l. 206: “contains a low value of WSS” – not sure what this could possibly mean. “a low value of WSS” should anyway read “a low WSS” as the last ‘S’ stands for ‘score,’ which is a value.

p. 6, l. 214: “calculates a WSS value of 0.69” better had to read “yields a WSS of 0.69”

p. 6, l. 223: “seasonal dynamic” should read “seasonal dynamics”

p. 7, ll. 257–262: I am quite surprised about this. I would expect that upwelling events not only bring up cold water, but also nutrient, DIC and TA rich waters. Unfortunately, the model description does not explain how the upwelling events are represented. Could you please elaborate on this.

p. 8, l. 281: “diatoms” should read “ diatoms’ ” (genitive)

p. 8, l. 295: “in-gassing” should read “absorption” or “uptake”

p. 8, l. 295: “variability” at which time scales?

p. 9, l. 329: “weaker” should read “lower”

p. 9, l. 354: I think that “counteracting” is more appropriate than “counterbalanced” at this point

p. 12, l. 424: “Environnenemnts” should read “Environnements”

p. 12, l. 432: “takes part” should read “is part”

p. 12, l. 434: “Agence” should read “Agency”

p. 12, l. 434: “from European” should read “from the European”

p. 12, l. 434: “used in this paper” should read “presented in this paper”

References

Baklouti, M., Diaz, F., Pinazo, C., Faure, V., and Quéguiner, B.: Investigation of mechanistic formulations depicting phytoplankton dynamics for models of marine pelagic ecosystems and description of a new model, Progress in Oceanography, 71, 1–33, https://doi.org/10.1016/j.pocean.2006.05.002, 2006a.

Baklouti, M., Faure, V., Pawlowski, L., and Sciandra, A.: Investigation and sensitivity analysis of a mechanistic phytoplankton model implemented in a new modular numerical tool (Eco3M) dedicated to biogeochemical modelling, Progress in Oceanography, 71, 34–58, https://doi.org/10.1016/j.pocean.2006.05.003, 2006b.

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