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Interactive comment on "A single-column ocean-biogeochemistry model (GOTM-TOPAZ) version 1.0" *by* Hyun-Chae Jung et al.

Anonymous Referee #1

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Review of A single-column ocean-biogeochemistry model (GOTM-TOPAZ) version 1.0 by Hyun-Chae Jung et al.

Summary.

The manuscript presents a version of the TOPAZ biogeochemistry model coupled to the GOTM water-column hydrodynamics model for the purpose of improving biogeochemical simulations in global climate models. The model is applied to a location in the East Japan Sea, and simulation results are compared with observations. Simulation results are also compared with results from a global 3D implementation of the original TOPAZ model coupled to a global circulation model MOM4.

General comments.

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Discussion paper

The manuscript is reasonably well-written, and mostly structurally sound, although a large part of the text in Results should be moved to Methods, and information is missing in a number of cases. I have a number of major concerns about the document, however:

1. Innovation. The manuscript only describes the model and a comparison with observations and with a 3D model. None of the broad-brush statements about the potential use and usefulness materialises.

2. Innovation. Other 1D ocean-biogeochemical models already exist. Why create another one? What can this one do that others can't? What can you do with this one that you can't do with others? How does the performance of this one compare with others?

3. Observations. No information about the observational data set is given. How was it collected? Where, and at which time/depth intervals? How was it processed?

4. Validation. The comparison with observations is mostly visual, and involves statements such as 'similar'. This must be made quantitative.

5. Validation. The text often contradicts the information in the figures, and suggests that the results are better than they really are.

6. Validation. Why are only anomalies presented in the time series? Anomalies compared to what? Does this mask biases?

7. Location. The location for application/validation was chosen because of its advective properties at the confluence of two ocean currents (p. 8, l. 8-9). This baffles me, as a water-column model can not (as the authors acknowledge) deal with horizontal advection. How can this site be used to reliably evaluate the model's performance? It's absolutely unsuitable. And indeed, most of the arguments given for mis-matches with the observations are related to advection...

8. Generality. The model is intended to serve very general purposes. However, it

is applied only to a single (unsuitable) site. How can we know that it is generally applicable to the purposes for which it was intended?

Most of these points are addressed further in the detailed points below.

Together, these issues are too many and too severe to repair in a revision cycle. Hence, I have to recommend rejection.

Specific comments.

p. 1, l. 20. reliably: requires quantification.

p. 1, l. 28-29. Requires reference.

p. 2, l. 1. Requires reference.

p. 2, l. 10. for differentiating. I'm not sure what's meant here. Probably not mathematical differentiation?

p. 3, l. 25. Please finish explaining all the variables before going into the equations.

p. 4, l. 25-26. empirical formulas derived from observations. Please expand. Formulas of what kind? Which observations? How were the formulas derived? Are they generally applicable, or (more likely?) specific to the location(s) where the observations were taken? How does this relate to the location used here? There's no need to repeat the Dunne et al. paper, but a summary is required here.

p. 5, l. 11-12. we used: How?

p. 5, l. 17. were determined: how? What was the source of the data?

p. 5, l. 26. process for calculating: please specify.

p. 5, l. 31. monthly average climate values. From which source? How can this be done without systematically enriching the system during the simulation?

p. 6, l. 7. [A]: please provide values and reference(s).

p. 7, l. 2,3: please explain what XI(lambda) and e(lambda) are.

p. 7, l. 1-4: are all these parameter value settings from Manizza et al?

p. 6, eqn 7. Why are contributions to the light-extinction coefficient by CDOM and suspended particulate matter not taken into account? These can be dominant in many locations.

p. 7, section 4.5. How was this used for the test case?

p. 8, l. 2. they: what does this refer to?

p. 8, l. 15. observational: These are not observational data, but model results. You can't verify a model with another model.

p. 8, l. 23. aforementioned observational data. Requires description of the data set.

p. 8, l. 30. similar. Please quantify. There are many occurrences of this kind of terminology, please find and address all.

p. 9, East Sea Intermediate Water. Should have been introduced in the description of the study area.

p. 9, l. 13-15: what do we learn from this?

p. 9, l. 16. Chlorophyll at 40 m. How do we know this is real? This is based solely on results of the current model.

p. 9, l. 24-25. attributed to horizontal advection. How do you know?

p. 9, l. 25-27. I don't see the logic. The 3D model has an influx of nutrients, but the 1D model has higher chlorophyll. How can this influx explain the difference? I would expect the reverse.

Figure 5 b,c. The model appears to be getting enriched with N and P during the simulation. Why? How does this affect the applicability of the model for the intended purposes?

p. 10, l. 1-2. Why February, August and 'the entire period'?

p. 10, l. 3. accurately simulated ... nutrient concentrations. I disagree. The averages of phoshorus and silicon near the surface are outside the standard deviation of the observations.

p. 10, l. 3. upper layer: how is this defined?

p. 10, l. 4-5. phytoplankton at 40m. No observational proof of this is presented.

p. 10, l. 7. each layer: which? how many? Please define all layers clearly.

p. 10, l. 8. properly simulated. I disagree, O_2 in the model is substantially higher than observed in the upper 80 m.

p. 10, l. 13. subsurface layer. How defined?

p. 10, l. 13. since. I don't follow the logic here. Were the model results and the observations not processed in the same way?

p. 10, l. 13-14. not in figure 6b between 0 and 80 m.

p. 10, l. 17. excellent. I disagree.

p. 10, l. 14. all within range. No. O_2 is outside the standard deviation below 300 m, and silicon over the entire profile.

p. 10, l. 22. reproduced. Well, it doesn't really, does it?

p. 10, l. 26. consistent. I disagree.

p. 10, l. 23. sensitivity experiments. Why were these not done here?

p. 10, l. 30. excellent tool. Please elaborate how.

p. 10, l. 31. parameterisation improvements. How? I don't quite see how this model, which has its own (unexplained, at least here) parameterisations, can be used to improve parameterisations of other models, which may well be incompatible.

p. 11, l. 1. many issues. Please specify. Should these not be sorted out first?

p. 11, l. 5. This: refers to what?

p. 11, l. 11. coupling ... more easily. How/why?

Figure 3: why not include the nutrients and oxygen here? The data from Fig 6 can be plotted in the first column as well; if sparse as coloured circles?

Figure 6: I'm a bit surprised that chlorophyll/fluorescence was not measured as well? If so please use?

Technical corrections.

p. 7, l. 16. anthropogenically. Remove this word.

p. 7, l. 29. Refer to Figure 2 here.

p. 7, l. 23 to p. 8, l. 24. This section is Methods, not Results.

p. 8, section 5.1. header can be removed.

p. 8, l. 27-28. This is Methods, not Results.

Abbreviations. There are so many abbreviations that the manuscript would benefit from a tabulated list.

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