

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

Anonymous Referee #2

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General Comments:

This manuscript is relevant to be published by the Geoscientific Model Development due to the approach of present a single-column ocean biogeochemistry model, GOTM-TOPAZ, as a tool for developing and test new methods to improve the ocean biogeochemistry models. As these models are essentials components in the Earth System Models, the development of tools to improve these models is necessary. Developments and improvements in the ocean biogeochemistry representation by the models are crucial for a better representation of all earth system dynamics.

The work is also interesting to be published because there were modifications in the marine biogeochemical model TOPAZ, as the insertion of a module to reproduce upwelling and also the representation of the air-sea gas transference for O₂ and CO₂.

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The paper is consistent because there was presented an evaluation of the performance of GOTM-TOPAZ by comparisons with observations. Another interesting point of this paper is that, as the model TOPAZ was separated from the MOM model this paper can inspire others studies testing TOPAZ with others OGCM models. Also, others applications with this single-column model could be done in the future.

In summary, I believe that this manuscript is important and deserves to be published. However, I suggest here some points that should be revised aiming to produce a final version in a better condition to be published.

Specific Comments:

Page 3, Line 10: The phrase “we selected points in the East/Japan Sea” is wrong, because in the paper there were just analyzed results for one point. At the page 7 line 22 it is said: “To verify GOTM-TOPAZ, we selected a point . . .”.

About this item, I believe it would be necessary to show results for more points. The study would be more robust if there were analyses for more points located in areas with different characteristics. For instance, it would be selected at least more two points to verify the model performance, one would be located in the East Korean Warm Current and other in the North Korea Cold Current. This approach would be more interesting, instead of just to select a point where the two currents meet, as was presented in this paper.

Page 4, Line 3: The section that explains the optical feedback is Section 4.4.

Page 5, Line 2: It is said that the MOM version is 5, however in Figure 4 in the legend it is written that it is analyzed results from MOM4p1_SIS_TOPAZ. Which is the correct MOM version used in this paper?

Page 8, Line 12: It is necessary to describe which are the data used for initializing the biogeochemical tracers in TOPAZ. Which are the data sets and sources?

Page 8, Line 27: Just 4 years of spin up for a biogeochemical model is enough? Most

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of the applications with biogeochemical modeling are based in long spin up periods.

Page 8, Line 30: This similarity between GOTM-TOPAZ and observations is just on the first 40 meters for temperature. The difference in deeper regions must be discussed in this point.

Page 9, Line 5: Similarly to the latest comment, it is necessary to be clear in the text that this correspondence in seasonality between the model GOTM_TOPAZ and observation are just in the initial 40 meters.

In Figure 3, there is no figure for observation to chlorophyll. In this case, I do not see a reason for this variable to be included in this figure.

Page 9, Line 13: These correlation coefficients are statically significant?

Page 9, Line 6: It would be interesting here to discuss why the model GOTM-TOPAZ does not represent well the temperature in deeper regions, especially below 80 m. This discussion would be more interesting with the inclusion, in Figure 3, of a figure with the biases between models (MOM and GOTM-TOPAZ) and observations. Maybe this deficiency in the deeper regions is related with a short spin up period.

Page 9, line 30: The phrase "These results can be viewed as validating the gas flux equation reproduced in GOTM-TOPAZ" does not make much sense, once the correlation coefficient for GOTM-TOPAZ was worse than for MOM. Again, the correlation coefficients presented in Figure 5 are statically significant? In this paper, there was no evaluation of the fluxes. It is possible to evaluate the CO₂ flux based on observational data, for instance, from SOCAT database.

Page 9, Line 21: The phrase: "In this paper, we have explained the major models that comprises GOTM-TOPAZ and the ocean biogeochemical process reproduced within the models" is not appropriated because you do not have made this on this paper. The model TOPAZ and the ocean biogeochemical process reproduced in this model was not explained on details on this paper. Actually, this explanation was not the main

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objective of this paper. To start the item discussion, I believe it would be more relevant to mention the main contributions of this paper, as a study about the development of a single-column ocean-biogeochemistry model.

Page 10, Line 28: In this paper was not presented results about this sensitive experiments that are exemplified, how can you affirm that GOTM-TOPAZ will be good in this kind of applications? In the discussion topics, you should dedicate to discuss based on the results found in the paper.

Finally, in the discussion, there was no evaluation about the upwelling representation. I believe that would be important to include in the paper the evaluation of the upwelling representation. How the module related to w-advection impacted the results? A comparison of the vertical movements reproduced by the model with observations would be interesting.

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