

We thank the constructive suggestions. The comments are fully considered and adapted in the revised manuscript accordingly. Below are our response to the comments one by one.

Reviewer #2:

A ROMS based model has been used in this manuscript to disentangle the physical and biological mechanisms involved in the development of the phytoplankton blooms recurrently observed in the middle of Taiwan Strait in wintertime. The objectives are commendable but the approach and presentation of the manuscript are quite far from covering the expectations created by the title.

The description of the coupled physical-biological model is so brief that it is tough to assess its potential to simulate the study case. The rates of transformation among the different nitrogen pools, the cornerstone of any biological model, are not reported anywhere. A table with the numerical values or the equations used to obtain the model parameters used in the manuscript and their corresponding ecological justification is mandatory.

Response: Accepted. We will add more descriptions of the models and parameter settings in the revised manuscript.

Furthermore, the biological model do not include a proper dissolved organic nitrogen (DON) pool, a key component of the N cycle of any coastal area. Note that the labile fraction of this pool contributes to N recycling and the semi-labile fraction to N horizontal transport. Therefore, it could affect dramatically the distribution of the different N pools, including Chl a. For example, exclusion of the DON pool in the model would lead to an overestimation of the importance of the vertical flux of organic N into particles compared with the horizontal transport of organic N in the dissolved form. I see that the biological model proposed by Fennel et al. (2006) and used in this manuscript include two pools of organic N that sink at different rates, but do not include a proper DON pool. Therefore, the authors should justify convincingly why DON was not included in the model of Taiwan Strait, or indicate if the small detritus pool is trying to mimic DON, or run again the model including this key compartment.

Response: We will give more descriptions of the biological model in the revised manuscript.

In the model, the small detritus is trying to mimic the DON pool, which can be re-mineralized into (inorganic nitrogen) NH_4 at a certain rate. To justify the contribution of the re-mineralized NH_4 to primary production, two sensitivity experiments (Tab 1) had been carried in the climatological model (Wang et al. 2013). The results show the chlorophyll distribution is less changed in winter, but is changed obviously in summer (Fig 1), which indicates the our study of the winter ecosystem in the TWS is not sensitive to the DON re-mineralization. The reason can be described in following.

In winter, the formation of the detritus is mainly at the coastal zones of the TWS corresponding to the high primary production. While the nutrient are abundant (over 15 mmol/m^3) at the coastal zones dominated by the MZCW. Hence, the supplement of the re-mineralized NH_4 is less important compared to the remote transported nutrient. In contrast, the re-mineralization process at the TWS is much more important in summer because of the low-nutrient status. The results in Fig 1 are corresponding with that in your question. While the sink rate is slowdown in Case 1 (Tab 1), more detritus are horizontally transported through the TWS, leading to the vertical supplement of the re-mineralized nutrient decrease. Then the re-production decreases and the chlorophyll

decreases in Fig 1b. Further, while the benthic re-mineralization process is shutdown, the bottom re-mineralized NH_4 is removed from the system, the chlorophyll decreases more obviously (Fig 1c), even at the upwelling zones marked by red circle in Fig 1.

Tab 1. Description of the sensitivity experiments

Experiments	Sink rate of small detritus	Sink rate of large detritus	Bottom re-mineralization process
Control run	1 m/day	3 m/day	With
Case 1	0.1 m/day	1 m/day	With
Case 2	1 m/day	3 m/day	Without

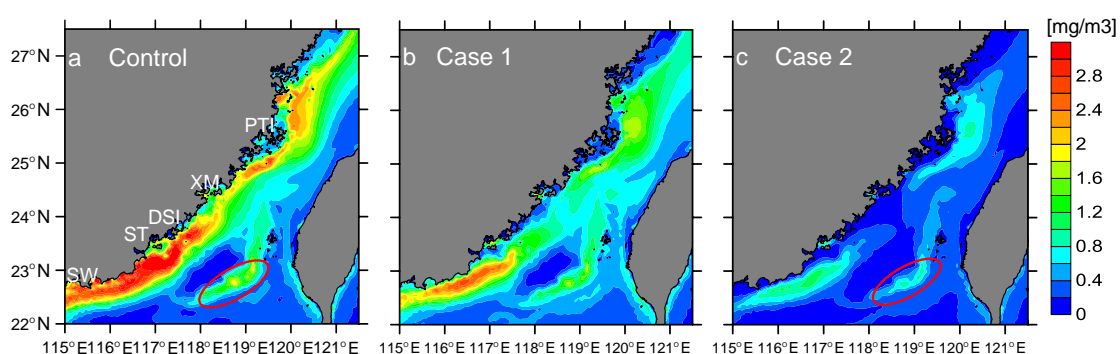


Fig 1. 0 ~ 40 m averaged chlorophyll distribution at the TWS in summer.

Finally, the reliability of a model is demonstrated by validating it against field data. This is something that the authors have not done in this work. Undoubtedly, this is the most critical weakness of the paper. Before studying the relative importance of the different components of the model to produce the simulated detachment of the winter phytoplankton bloom from the coast to the middle Taiwan Strait, it is compulsory to validate the model with field data. And this is something that can be done with the in situ and satellite data presented in the manuscript.

Response: Accepted. The model validation will be added in the revised manuscript as shown by the Fig 2.

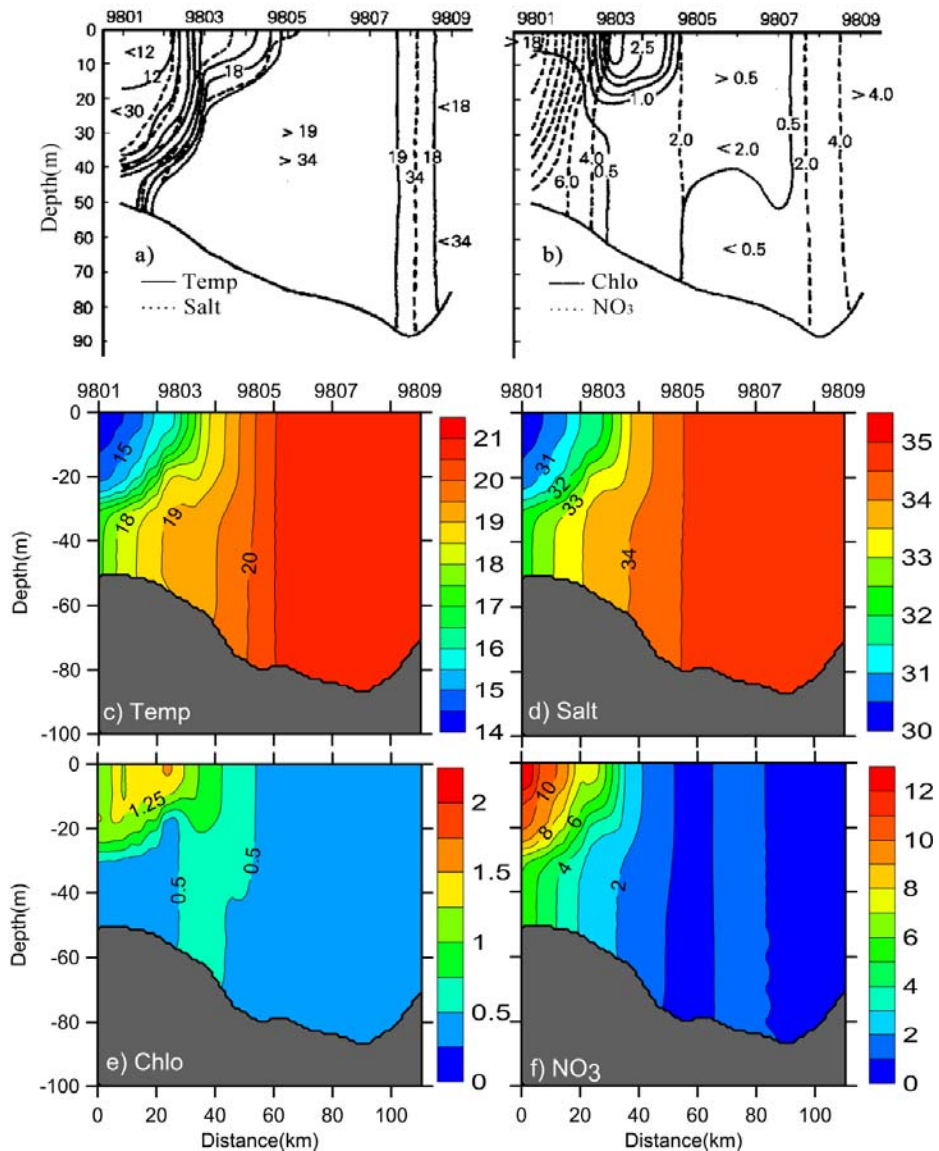


Fig 2. Comparison between in-situ observation (top panel) and the model result (middle and bottom panels) in 1998.

In summary, I cannot recommend publication of this manuscript in Biogeosciences in the present form. But I would encourage the authors to revise the model and validate it to produce a thoroughly revised version of the manuscript.

Response: Thank you for your encouragement. We will revise the manuscript by fully considering your suggestion.

Reference

Wang, J., Hong, H., Jiang, Y., Chai, F., and Yan, X.-H.: Summer nitrogenous nutrient transport and its fate in the Taiwan Strait: A coupled physical-biological modeling approach, *Journal of Geophysical Research: Oceans*, 118, 4184-4200, 10.1002/jgrc.20300, 2013.