

Interactive comment on “Mechanism for initiation of the offshore phytoplankton bloom in the Taiwan Strait during winter: a physical–biological coupled modeling study” by J. Wang et al.

Anonymous Referee #2

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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

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parameters used in the manuscript and their corresponding ecological justification is mandatory.

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.

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.

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.

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