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Comment on bg-2022-152

Anonymous Referee #3

Referee comment on "Role of phosphorus in the seasonal deoxygenation of the East China Sea shelf" by Arnaud Laurent et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-152-RC3, 2022

Review of Biogeosciences manuscript "Role of phosphorus in the seasonal deoxygenation of the East China Sea shelf" by Arnaud Laurent and Coauthors.

The manuscript presents a modelling study of the effects of nutrient limitation on deoxygenation in the East China Sea shelf. The manuscript is well composed, and results are clearly presented using meaningful figures. In particular, the conceptual scheme presented in Fig. 11 well resume the main results of the manuscript. Thus, my suggestion is to publish the manuscript after minor revisions (listed hereafter).

- 75. Consider changing "simulations with and without P" in "simulations with and without P limitation".
- 79. I suggest specifying that the scenario simulations are all made with reduction of nutrient loads.
- 91-102. Even if references are provided, maybe some additional details on the simulation would be helpful for the reader. For instance, are the circulation and the biogeochemical model coupled online or offline? Since the simulation results can be in principle affected by any nutrient inputs, information about treatment of nutrient fluxes at boundaries and at surface could be of interest for the reader.
- 106-108. My suggestion is to somehow revert how the noPlim simulation is introduced by firstly clarifying that the objective is to have a simulation without P limitation, and then by specifying that this has been implemented disabling P in the biogeochemical model.
- 110. Maybe the final sentence of the paragraph ("Otherwise […] simulation") can be removed if the previous suggestion 4 will be accepted by the Authors.
- Sub-section 3.1 is inserted in the Results section. However, since riverine input is a forcing of the simulations, I am wondering if it would be more appropriate to move 3.1 in section 2 (Methods).
- 130-135. Validation is made considering observations limited to the surface layer. I recognize that validation can be made only with available data and that the main aim of the manuscript is not the validation of the model, however the Authors could introduce in the Discussion some comments about the implications of the use of a relatively

limited dataset or the expected impact of additional observations.

- Figure S1. If I am not wrong this figure shows the same results of Fig. 2 of Zhang et al. 2020. I think that it should be somehow highlighted that this material has been already published elsewhere.
- 191-192. Since only regions 2, 4 and 5 are shown in Fig. 7, I suggest removing the "e.g." used into the brackets: "more concentrated in the smallest area (zone 2) and more diffuse in the larger areas (zones 4 and 5)."
- 231. Maybe it would be clearer "The N+P and N-only reduction strategies" instead of "The 2 strategies".
- Figure 6. Even if one can guess that the increasing numbers on the horizontal axis are the distance along the CE-JI line, it should be better to specify this aspect in the figure or in the caption.
- Figure 7. I think that it could be useful to specify that the change is related to P limitation.