Ocean Science Discussions

Ocean Sci. Discuss., referee comment RC2
https://doi.org/10.5194/os-2021-35-RC2, 2021

Comment on os-2021-35

Anonymous Referee #2

Referee comment on "Can assimilation of satellite observations improve subsurface biological properties in a numerical model? A case study for the Gulf of Mexico" by Bin Wang et al., Ocean Sci. Discuss., https://doi.org/10.5194/os-2021-35-RC2, 2021

The manuscript by Wang and Coauthors investigates the impact of the assimilatio of satellite surface observations to both physical and biogeochemical variables in a coupled model of the Gulf of Mexico physical and biogeochemical dynamics. Independent (non-assimilated) profiles form five BGC-Argo floats are used for validation. Results provided in the manuscript highlight interesting aspects on the capability of DA (data assimilation) to effectively correct ocean simulations, on the difficulties arising in coupled physical and biogeochemical assimilation, and on the relevant role played by prior model calibration in DA.

The manuscript is well written and of interest for the scientific community considering recent and foreseen upgrades in physical-biogeochemical ocean DA. My review is limited to few points (most of them minor) that I think will further improve the manuscript quality.

Since line numbers are corrupted in the manuscript file, in the comments hereafter they are indicated by # followed by the part of line number visible in the manuscript. Page numbers are also provided together with line numbers.

- I suggest to introduce the alternative parametrization of the light absorption in a different way. In the manuscript it is currently described as an alternative that has been considered after investigating the results of previous simulations. I think that presenting this formulation as an alternative since the beginning would better emphasize the role of prior model calibration. Thus, I suggest to describe the alternative formulation not in a temporal framework (i.e., without specifying that it has been applied *after* previous simulations) as it is currently done in the abstract and in the manuscript sections. In particular: i) the first paragraph of Section 3.3 could be moved and adapted to Section 2.4; ii) in Section 2.4 the Authors could indicate that five (instead of three) simulations were performed; iii) it can be further stressed through the manuscript that the alternative formulation for the light absorption was adopted to investigate the sensitivity of subsurface DA impacts to model calibration (and in particular to the light penetration formulation); iv) the abstract should be adapted accordingly.

- I think that the comparison with independent BGC-Argo floats is a valuable aspect of the manuscript, however, it would be interesting to know the spatial-temporal distances of the non-assimilated profiles with respect to the assimilated ones. Did they cover similar areas of the gulf of Mexico? And in the same period? In my opinion clarification on this aspect would help to better understand and comment the relatively small impact of Argo profiles assimilation when compared to the independent BGC-Argo data. Moreover, this could help also in commenting the differences between the two maps of Fig. 6 (are the differences mainly located close to assimilated Argo profiles?).
- I suggest to insert some comments in Sec. 3.3 about the impact of the alternative parametrization on RMSE with respect to satellite chlorophyll. Results of tab. 2 show that RMSE in Free_alt is slightly higher than in Free, but on the other hand the improvement due to the assimilation (DAsat_alt) is relatively higher. I think that this point could be further highlighted and commented in the manuscript.
- From the sentence at lines 18-19 (p. 1) in the abstract it seems that the model was tuned using BGC-Argo in the present. However, the tuning was made in Wang et al. (2020) (lines #08-#09, p. 4). Tha Authors should consider to rephrase the sentence in the abstract.
- 16 p. 1. I think that the correct term for biogeochemical Argo floats is BGC-Argo instead of BGC Argo. Please, check other occurrences in the whole manuscript.
- 94 p. 4. Probably Figure should be abbreviated with Fig. (please, check other occurrences).
- #09 p. 4. Concerning the re-tuning of the half-saturation constant of nitrate, was it is done similarly to Wang et al. (2020), i.e. based on BGC-Argo? If not, could you explain how the updated value of the parameter was obtained?
- #32 and L. #36 p. 5. I suggest to check if Equ. Is the correct abbreviation for Equation in Ocean Science Journal.
- #53-#54, L. #61-#62 p. 6 and L. #58 p. 7. How were the values of observation errors defined? I suggest to add some references or details on criteria used.
- #88 p. 7. Probably an *and* is missing in the sentence: zero mean *and* variance of 1.
- #74 p. 11. I am not sure that the term *subsurface* is fully consistent here. Indeed, I would say that the model fails to simulate the high spatiotemporal variability also close to the surface, more generally the whole euphotic layer is affected by the issue.
- #94 p. 11. How were the two light attenuation parametrizations calibrated? Could you provide some details or references about?
- #16 p. 12. Since the large use of BGC-Argo floats for model validation demonstrated in Salon et al. (2019), consider if it is relevant to add it in the listed references.
- #62-#63 p. 14. As far as I know, in Goodliff et al. (2019) the muting of the multivariate update concerned not only chlorophyll but all the phytoplankton variables (whilst multivariate updated was maintained for nutrients and oxygen).
- Fig. 10, I suggest to insert measurement units (at least in the caption).
- Accordingly to comment 1, I think that in Discussion and/or in the Conclusion the need of an a priori well calibrated model could be further stressed by the results obtained using the alternative light attenuation parametrization, since it is an example of how DA benefits can be limited by a parametrization that it is not fully consistent with the modelled processes.