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Comment on gmd-2021-20

Anonymous Referee #2

Referee comment on "The Lagrangian-based Floating Macroalgal Growth and Drift Model (FMGDM v1.0): application to the Yellow Sea green tide" by Fucang Zhou et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-20-RC2>, 2021

This manuscript presents a Lagrangian-based ecological model tool that reproduces the spatial and temporal dynamics of *U. prolifera* macro-algal blooms observed in the Yellow Sea during the spring to summer period. The floating *U. prolifera* are represented as Lagrangian synthetic particles of a certain mass whose drift is calculated using the surface velocity output of a regional ocean circulation model (FVCOM) to which a windage component is added by taking a percentage of the wind speed at 10m from the ECMWF wind product. An ecological model, embedded within each particle, controls the growth dynamics of the *U. prolifera* based on the environmental factors of salinity, temperature and irradiance, leading to duplication or merging of particles. The experiment seems to reproduce satisfactorily the northward propagating bloom development with peaks in biomass around June-July to the north of the domain near the coastal regions of the southern side of Shandong Peninsula, as observed by satellite data for the years 2014 and 2015. Yet it fails to capture the year to year variability seen in the satellite product – in magnitude and extent –, suggesting important drivers of *U. prolifera* are not represented.

Overall, the structure of the paper could be improved, so does the writing as abbreviations and typos are presents.

I do not think that the experimental set-up is sufficiently convincing to ascertain that these results are robust.

Namely:

- the validation of the Lagrangian transport for supporting the choice of ocean model and fraction of the wind component for the windage is not robust; the authors set windage on 3.5% without having completed a sensitivity analysis or referencing a study that did so. Macroalgae are influenced by wind yet the fraction depends on the physical

characteristics of the floating object. The passive drifting experiments should be carried out with a much larger number of particles in the order of tens of thousands (currently only 6), and released over a wider period (currently simply a single date instead of across a full month of *prolifera* observed presence in the release area). Density maps of particle positions after 120 days for each experiments (ocean currents only, and different windage factors) should be provided to illustrate the differences in drifting patterns, together with a thorough argumentation for the choice in the balance of ocean current and windage for *U. prolifera* drifting. I also suggest the use of the Global Drifting Program dataset (if sufficient floats are available in the area) to evaluate drifting patterns in the region using drogued vs undrogued surface floats (that remove or include a wind factor), and evaluate the model skills in reproducing the patterns from observations. Such work would also benefit the interpretation of the patterns obtained with the individual based growth model, in order to disentangle influence of biology and physics to explain biomass distribution.

- The *prolifera* drifting experiment is carried out with an insufficient number of particles to capture the full ocean model variability, failing to convince the reader that the patterns obtained are robust. What's the rationale for deploying only 47 particles (each representing 100 tons of biomass), i.e. at 0.1° resolution on a single date (1st May), instead of for example deploying a particle in every gridcell of the model (resolution of 0.5 to 3km), together with a lower biomass per seeded particle, in that region, and repeated across the full period when *U. prolifera* is present in the area (mid-april to mid-May)? Are the results numerically stable with such low number?
- The ecological model could be better explained, in relation to the external factors chosen (and those disregarded) and enable the reader to get an understanding of how these factor influence growth/death. An equation of the *prolifera* dynamics over time as a function of these factors is needed. It seems though, that the model dynamics is simply driven by the laboratory-based measured rates for different ambient conditions, and that no physiological model exists. If this is not the case, it should be better explained, together with a sensitivity analysis of the model parameters.. Nutrients are known to influence macro algae growth, why is nutrient uptake not considered, using for example a biogeochemical model like in Brooks et al. (2018, Marine Ecology Progress). Only temperature causes mortality. What's the need for including salinity?
- The model reproduces the bloom dynamics to first order, yet little year to year variability is present in the spatial patterns for the two years of the study, although a clear inter-annual signal exists in the observations. For instance large parts of *prolifera* patches in July 2018 are uncaptured by the model. It is not possible to determine what is the cause for these discrepancies (biological or physical?). I wonder whether a better choice of particle deployment at start (as mentioned above) would improve the results. In support, we see that short term simulation (1 week) show better skills. Could this be explained by a better initialisation? An analysis (or inverse modelling approach) would be useful to get a feeling of the sensitivity to the choice of parameter values (whether physical or ecological) for best recreating the observed biomass concentrations.

A coupled growth and drift model is an interesting development, but in its current form it is not possible to assess the capability of the model. For all these reasons, I cannot support this manuscript for publication in GMD.