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Interactive comment on "Tidal impacts on primary production in the North Sea" *by* Changjin Zhao et al.

Anonymous Referee #1

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General Remarks: The paper investigates the impact of tides on the distribution of phytoplankton and other ecosystem parameters. For this purpose, three scenarios were conducted with the well-established ecosystem model ECOSMO, i.e. a non-tidal scenario, a tidal scenario (M2 and S2 considered) and a M2 scenario (only M2 considered). In order to also demonstrate the existing inter-annual variability, the model simulation was conducted for a 25 years' period (1990 to 2015). The overall impression is that the paper is written very carefully and in a clear and concise way. In particular, the scenario tests were chosen nicely to convincingly illustrate the impact of tides on the North Sea ecosystem dynamics. However, I have one principle concern, which should be considered by the authors before publication can be envisaged. As stated by the authors, the impact of tides is mainly governed by two counteracting processes.

On the one hand, an increased turbulence can transport more nutrients from deeper layers into the upper euphotic zone, which increases primary production. On the other hand, tides will increase the suspended particulate matter (SPM) concentration in the water column, which due to the shading effect will decrease primary production. Unfortunately, in the current paper version, the latter process is not described adequately. The present limitation to only organic SPM is not acceptable to describe the dynamics of suspended matter realistically enough, in particular in the southern North Sea. There are numerous publications, in particular those, which make use of remote sensing data, showing an important contribution of inorganic SPM input by rivers but also due to cliff erosion along the English east coast. From satellite observations it can be derived that these contributions dominate the SPM dynamics in the southern North Sea (see e.g. Pleskachevsky et al. JPO 2010). I would strongly suspect that ignoring the effect of inorganic SPM leads to an overestimation of the tidally induced SPM effect, since the background SPM concentration is too small, if only inorganic SPM is considered. Altogether, the entire presentation of the SPM dynamics is not acceptable as it is in the current version. As stated, compared to their reference paper Daewel & Schrum (2013), the implementation of the SPM dynamics was significantly modified. If this is the case, a thorough validation of this strongly modified scheme is indispensable, in particular since SPM dominates the light attenuation. The latter belongs to one of the two major processes affected by the tidal forcing, and therefore should be in the focus of the current paper. No figures or even numbers regarding the SPM concentration are provided. Therefore, it is not even possible to judge whether they are in the correct range at least. A general criticism of minor importance is the missing predation by fish and higher trophic levels. This deficit is only mentioned in the conclusions. However, a more serious discussion of this aspect would definitely be appropriate, in particular since it was noted in line 129 that the predator - prey interaction is considered, which at the first glance is even misleading. In summary, after the effect of inorganic SPM will have been considered appropriately, the manuscript will be suitable for publication in Earth System Dynamics after a moderate revision, accounting for the few minor remarks given below.

Detailed Comments: Line 129: It is not clear that the mentioned predator - prey interaction just concerns zooplankton and phytoplankton, whereas fish is not considered. This must be clarified. Line 138: The term “southern coast” should be specified more clearly. Line 280: The sentence is not clear. How can the “energy” of tidal currents interact with the atmospheric forcing? Moreover, it is not clear whether this specific interaction process is considered in this study. I guess so, but however, this should be stated. Line 540: Obviously, the difference to observation is larger than one order of magnitude. The arguments, which are presented to defend this inconsistency are not fully convincing to explain such a very large discrepancy. In particular, the argument given at line 547 that observations over a few days between July and August cannot be compared with seasonally averaged model data is not acceptable. It should be easy to extract the actual observation period from a 25 years' model results data set. By this means a direct comparison could easily be performed. Line 552: Please correct to “close to”.

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