

## ***Interactive comment on “Seasonal hypoxia in eutrophic stratified coastal shelves: mechanisms, sensibilities and interannual variability from the North-Western Black Sea case” by A. Capet et al.***

**Anonymous Referee #2**

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The manuscript investigates the drivers of coastal hypoxia. It couples hydrodynamic, benthic, and biogeochemical models, as well as suggests a statistical model with the ambition to be used in management practice for predicting the development of hypoxic conditions. The regression model, which uses nitrogen discharge, duration of stratification, and winter stock of labile organic detritus as predictors, is compared against observations. A hypoxia index is introduced as a numerical measure for such comparisons.

The construction of the statistical model for predicting the occurrences of hypoxia is novel and potentially very useful. The study is topical and the results should be of

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interest to a wide community of both researchers and environmental managers. In order to be fit for publication, however, the manuscript needs to provide more details about the design of the study, as described below, particularly in point 2 below.

1. The quality of English can be improved in places, particularly the choice of words.
2. The setup of the model is not clear enough. The individual components (the hydrodynamic model, sediment model, and biogeochemical model) are explained by giving references to previously published works. It is not clear whether those models are used unchanged and whether any site-specific parameterizations are used in this study. Equally important, the coupling between these model components is not described. One can imagine that this is not a trivial task and more information is needed to assess this work. From the text of the manuscript it is not even clear if the modeling results used in this study are original or were taken from the cited studies. If original results are used, a section on modeling results is needed before proceeding to assessing the model performance by comparing it to observations (section 3).
3. The statistical approach for estimating the model's suitability appears good.
4. The criterion on the Brunt-Vaisala stability frequency is not clear. The stability frequency varies with depth. At which depth is it taken here? At maximum? Similarly, a better definition is needed for the density difference that is used in defining the mixed layer depth:  $\Delta\rho$  over which depth interval?
5. A hypoxic index  $H$  is suggested as an indicator of the severity of hypoxia, which combines both the area and duration of hypoxia. The attempt to introduce such an index (presumably to be used in management practice) is laudable. It may be worth specifying, however, that some of the effects of hypoxia are not linear with the duration of the hypoxic episode, which may be important if such an index is accepted in management practice. Also, one can imagine that in some instances normalizing by the total area, as well as by the entire time period (one year), may be beneficial, as it produces a nondimensional index that could be easier to compare among environments.

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6. The definition of “the winter sediments stock of semi-labile detritus C” needs to be better. What are the units, what is ‘semi-labile’, what role does sediment resuspension play (i.e. would you define it in the same way in deeper waters)? Fig. 12 uses C in mmolC/m<sup>2</sup>, which implies a vertically integrated quantity. Over which depth is the integration carried out? The stated values 10 mmolC/m<sup>2</sup> are very small compared to the total pool of labile carbon in the surface decimeters of sediment.

7. The regression model is defined so that it uses predictor values that are normalized by their mean and variance (Eq. 3). Does this imply that the occurrence of hypoxia is considered to depend on the deviation of nitrate loadings, temperature, and stratification strength from their average values but not on their absolute values? One would think that, for example, large percentage variations in nitrogen discharges in clean oligotrophic systems may have smaller effects than relatively small variations in a system that is already on a brink of hypoxia. This seems like an issue that can complicate transferring the developed model between environments. It is also confusing that the regression model in Eqs. 3 and 7 uses the normalized H\* whereas the figures use non-normalized H.

Minor criticisms:

8. Fig. 2: Caption for panel (a) is missing a number for the hypoxic threshold: “[O<sub>2</sub>] < X? mmol/m<sup>3</sup>”.

9. Fig. 7. The potential energy anomaly is badly defined as “the amount of energy needed to mix the entire water”. I believe it is essential to say that it is the volume-specific difference in potential energy between stratified and mixed state and that it is averaged over the entire water column (is it?). Otherwise the standard units of J/m<sup>3</sup> don’t match the definition, which in its current form implies J/m<sup>2</sup>.

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