Biogeosciences Discuss., 9, C8669–C8673, 2013
www.biogeosciences-discuss.net/9/C8669/2013/

Biogeosciences
Discussions

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.

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Received and published: 1 March 2013

This paper investigates the mechanisms causing interannual variability in hypoxia on the North-Western shelf of the Black Sea. The manuscript falls apart in two parts. In a first part, a 3D coupled hydrodynamic – biogeochemical model is presented, while the simulated fields of O2 are compared to available point observations. In a second part, a hypoxic index H is introduced, which is subsequently linearly regressed against the model output, in order to obtain a simplified empirical model.

The topic of this ms is entirely suitable for Biogeosciences, the modelling approach appears sound to me (although this is difficult to assess with the limited information on

model formulation and parameterisation provided in the manuscript). The subsequent analysis of the model output, which separates the climatic effects from the nutrient effects on the interannual variability of coastal hypoxia is creative and innovative. The message that undersampling can provide a strongly biased view on the occurrence of hypoxia (Figure 10) has direct and important repercussions for monitoring and coastal zone management.

Still, the manuscript suffers from a number of shortcomings, as detailed below. Once these are addressed in the revision, the paper is suitable for publication in Biogeosciences, and will form a valuable contribution on the mechanisms and drivers causing coastal hypoxia in the Black Sea.

Major comments

[1] The paper suffers from wrong word choices and poor English. This obstructs the reading flow and sometimes it is unclear what is actually meant by a given sentence. The problems occur throughout the text and are too numerous to be listed. In the revision, the text should be thoroughly screened and improved (e.g. by a English).

[2] The model description in section 2.1 is very, very brief, and it's not highly informative. This way, the model basically remains essentially a black box to the reader (which I personally don't like when reading modelling papers). This especially pertains to the biogeochemical model: it impossible to get a feel of how simple or complex this biogeochemical model formulation is. For example, I think the sediment domain only covers the shelf, and not the whole Black Sea.

Similarly, no information at all is provided on the parameterisation of the model. It would be highly coincidental that all parameters exactly the same as in Gregoire et al (2008)? For example, the resuspension description is not in in Gregoire et al (2008).

In principle, scientific work should be repeatable as to verify the conclusions. Based on the limited information, the model simulations presented here cannot be repeated.

Although I don't advocate to repeat all the details from previous papers of Gregoire et al. (2008) and Capet et al. (2012), the current description in section 2.1 hardly provides any insight into the model. Therefore, I would advocate more detailed description of the model formulation as well as a more detailed description of the parameterisation in an appendix.

[3] The model output for oxygen is compared to point measurements available in the WOD and BSC databases. This is done in a systematic way, by first introducing the model skill metrics in section 2.3. and then doing the model data comparison in section 3 (I like the question and answer format of this section).

However, the O2 is just one of the many state variables. How does a model data comparison work out for these other state variables? For example, the temperature of the water column in spring is critical (March SST features in simple model for hypoxia index H). How good is the model at representing March SST? No information is provided here, and neither is it discussed whether such a model data comparison has been done in any of the previous papers.

[4] An important novel message in this paper is that accumulation of organic matter in the sediment during hypoxic periods may aggravate future hypoxia. This is illustrated in Figure 12. However, this conclusion has to be thoroughly checked, as the figure 12 is probably wrong. A typical accumulation of 10 mmol C m-2 (see figure 12) and a decay constant of 0.1 yr-1 for semi-labile organic matter provides an extra oxygen demand of 1 mmol C m-2 yr-1. This number is way too small to have an impact of future hypoxia. Typically, sediments have an oxygen demand of 2000 – 10000 mmol C m-2 yr-1, so the extra organic matter accumulation only contributes 0,025% or less to the O2 demand. The units of figure 12 are probably wrong.

Moreover, is there any observational evidence of accumulation of organic matter in Black sea sediments to support this conclusion that org C aggravates future hypoxia?

[5] hypoxia index

In my view, the hypoxia index defined in Equation (1) can be defined in a much more meaningful way. Right now it has strange physical units (unit of area times unit of time) and is not directly interpretable. However if one would properly renormalize H for the integration period and define it as:

H = 1/T (Integral of A(t) from 0 to T)

If T= 1 year then H would denote yearly averaged hypoxic area. If one further normalizes for the total shelf area A_shelf

H = 1/(T*A_shelf) (Integral of A(t) from 0 to T)

This way, one obtains a hypoxic index between 0 and 1, defining the yearly averaged fraction of the shelf that becomes hypoxic.

[6] P18415 It has been found. . . No details are given why the index H only depends on 4 variables and how this selection procedure has been performed.

[7] Why does fig 13 need a power law? A linear regression seems fine to me.

[8] A comment on the practical use of index of the H index for predicting future hypoxia. Two parameters are easy to measure or constrain (March SST, annual nitrate dicharge), but the other two are not easy to determine (winter stock of sedimentary semi-labile carbon and duration of stratification). The latter hence impede the practical use of the H index for predicting future hypoxia.

Minor comments

Title: "Sensibilities" is a wrong term here, probably one means "sensitivity". I would suggest to change the title to: Interannual variability of seasonal hypoxia on the North-Western shelf of the Black Sea

The hypoxia threshold of 2 mg L-1 corresponds to 62 mmol m-3. A few times, it is referred inconsistently equalled to 65 mmol m-3

Table 1. What is the difference between symbols Z and z? Why do they differ? Why are

18398 L3 to compensate : wrong word choice

Repartition: wrong term used many times throughout the ms -> change to “distribution”

18400 L15 modifies the sedimentary geochemical cycling through the removal of bioturbating infauna

P18404 L 11 I don’t understand : horizontal variables ?

P18404 L 25 represents 3Gmol yr-1 of what?

P18404 L 27 “imposed as fixed concentrations” what is meant by this?

P18409 L2 Fig5a too small plots

P18409 L14 The reader has no idea where Chilia and St-George are (no map is provided)

P18409 L25 Why was this particular site A chosen? Explain

P18410 L4 Plume of which river?

P18410 L20 Is this referring to model results? P18411 L7 Change DOX -> O2 P18411 L14 taken up P18411 L20 wind speed (hence... P18411 L23 Give the influx and outflux, and then the net flux (2 Gmol yr-1), so one can compare

P18414 L19 misfits -> residuals

Interactive comment on Biogeosciences Discuss., 9, 18397, 2012.