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Biogeosciences Discussions

Interactive comment on "Annual hypoxia dynamics in an enclosed gulf" *by* K. Kountoura and I. Zacharias

Anonymous Referee #2

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GENERAL COMMENT

The stated goal of this manuscript is to understand how oxygenation of the bottom waters occurs without vertical mixing in the Amvrakikos Gulf. The authors describe the physical controls on oxygen conditions in this gulf based on a year's worth of observations and an associated model. They concluded that seasonal changes in the density difference between the gulf and adjacent, open sea drive a seasonally-varying horizontal intrusion of oxygenated water from the adjacent sea. This horizontal transport is important because low wind-stress and tidal energy allow this system to be highly stratified year-round. Before I continue, I should note that the authors refer to the gulf as "enclosed". This can't possibly be the case if there is horizontal exchange with an adjacent water body - so I assume they mistakenly used an inappropriate word. Please

use "semi-enclosed" or maybe even fjord?

This manuscript describes controls on dissolved oxygen dynamics in a type of system (fjord-like) that is somewhat understudied (in terms of oxygen) relative to partially-mixed estuaries, continental shelves, and large inland seas. Even though the oxygen conditions in many fjords have been studied (e.g., Nutt and Coachman 1956, Journal of the Fisheries Research Board of Canada; Richards et al. 1965, Limnology and Oceanography; Nordberg et al. 2000, Journal of Marine Systems), many of these systems are located in regions with strong winds and tidal fluctuations. Thus, a publication about the dynamics of Amvrakikos Gulf is somewhat unique and useful because physical stress/energy is low. That said, this is a highly descriptive paper that highlights a model analysis where very little of the model output is presented and analyzed. The paper is quite short, and could easily be doubled to more completely present the model details and perform analysis of model data to support the conclusions of the paper. The results of this manuscript should be more thoroughly discussed in the context of previous research from other fjord-like systems.

Thus, although I support the publication of this data in some form, the current manuscript requires more analysis of model output (e.g., a salt (and oxygen) budget including advective and diffusive terms, simulation for a period longer than 2 months, statistical evaluation of model performance). I suggest this analysis be performed and the paper re-submitted for another full review.

SPECIFIC COMMENTS

Because I have suggested such a substantial revision, I will not comment on small editorial changes to the manuscript, but I will focus on more important issues.

(1) Line 17-22: The sentence seems to be redundant: "Among the most important causative agents which can cause this imbalance is the interaction between nutrient overload, resulting in eutrophication, low physical energy, fresh water inputs, stratification, changes in oceanic circulation, reduced winter ventilation, global warming, high

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residence time, and limited exchange and loss of sediment-buffering capacity (Druon et al., 2004; Gilbert et al., 2005; Conley et al., 2009; Kemp et al., 2009)"

“Stratification”, “reduced winter ventilation”, and “low physical energy” could be represented by a more general term, like "vertical mixing". “High residence time”, “limited exchange”, and “changes in oceanic circulation” might also be consolidated into one term.

(2) Section 2.3: The model that was employed for this analysis must be much, much better described. I understand that you should not review the entire model in detail, but several important aspects of the simulation are omitted. Is there an article in the primary literature that describes this model? If so, it should be cited. (a) What is the time-step of this model? (b) Include a plot that illustrates the grid (c) Where did the morphometric data come from? (d) What equipment was used at the weather station? (e) How was atmospheric forcing based upon one station applied throughout the model domain? Constant values across space based on the 1 station? (f) Were the inputs from the two main rivers included in the model forcing? (g) What turbulence-closure scheme was used? (h) Include a description of the terms in equations 1-6 (i) Hydrodynamic models have many parameters that are not described here. The values of these parameters are important to know (e.g., horizontal diffusivity?, bottom-stress?) (j) What parameters were altered during the calibration? (k) Why was the model only run for 2 months, when you have a year of data to calibrate it? Computational limitations? Even if it took 3 days to run one year, a one-time run of a year would be worth it and allow you to actually analyze the seasonal changes in inflows with the model? (l) Figure 7 compares the model and the data for salinity. Are the x-axes the same for these figures? Can you use grey scale? (m) Why not compute a statistical rubric for the data, such as RMSE, where you have profiles?

(3) As I mentioned before, the model data should be more thoroughly analyzed. Compute a salt budget from the model to understand the seasonal changes in salt flux due to vertical mixing and horizontal advection. The transport terms, if aggregated, could

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be coupled to the observed oxygen distributions to compute regional oxygen budgets to understand the balance between advection and diffusion. Perhaps a simple formulation of biological oxygen uptake (e.g., Hetland and DiMarco 2008. Journal of Marine Systems 70:49-62) could be coupled to this model to understand the balance between physical and biological oxygen terms.

(4) At the heart of this paper is the discovery that a seasonally-dependent inflow of oxygen is the strongest means of oxygen replenishment in the gulf. The resulting 3-layer structure in the vertical oxygen profiles is an interesting and important result of this (Figure 5) that should be highlighted. It seems from this distribution of oxygen that a 2-layer circulation has developed beneath the pycnocline during January and March, where the high oxygen water penetrated the deep water and the low oxygen water migrated eastward at mid-depths (Fig. 5). Does the model explain this? You can use the model transports to highlight this if you run the model for a year. Is there mixing across the low oxygen and high oxygen boundary at $\sim$25 m in January?

(5) I realize Figure 8 is used to conceptually illustrate the seasonal changes in water mass movement and associated oxygen levels. But I think an important point is lost here. With such large inflows through a single channel, volume balance would demand that an equally-large transport leave the gulf in the surface water. This is not indicated in the figure. The authors instead use a simple, seasonally-invariant exchange arrow to include tidal flow in the surface outflow – and this misses the point. Only include tidally-averaged flows in this diagram (tidal range is small anyway) and use the model-derived transports to scale the size of the in and out arrows. You could also do a simple Knudsen-type salt and volume balance to validate the model net in and outflows.

(6) Please include a section where your findings are discussed as similar to, and different from the dynamics in other fjord-like systems, based on a literature review.

Interactive comment on Biogeosciences Discuss., 9, 5049, 2012.