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# *Interactive comment on* “The Black Sea biogeochemistry: focus on temporal and spatial variability of oxygen” *by* E. V. Stanev et al.

**S.K. Konovalov (Referee)**

sergey_konovalov@yahoo.com

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Review on the manuscript bg-2013-606 "The Black Sea Biogeochemistry: Focus on Temporal and Spatial Variability of Oxygen" by E. V. Stanev, Y. He, J. Staneva, and E. Yakushev

The major issue of this manuscript is addressed to temporal and spatial variability in the oxygen distribution in the Black Sea waters. This issue is extremely important due to the ongoing process of de-oxygenation of or remarkable oscillations in the content of oxygen as the result of eutrophication and climates changes, and due to the very important role of oxygen. Dramatic changes in the distribution of oxygen in the Black Sea waters have been well documented and intensively discussed. Yet, the aimed analysis of the temporal and spatial dynamics of the suboxic zone, for example, has never been numerically performed and the role of processes governing this dynamics has not been numerically studied, too. Thus, the manuscript addresses relevant scientific questions within the scope of BG.

The manuscript does present novel ideas and tools. Specifically, the suggested combination of 1-D and 3-D models looks productive to study the oxygen dynamics and makes possible to effectively analyze the oxygen dynamics versus the thermohaline dynamics.

The major drawback of this manuscript is that the authors have raised too many questions and tried to discuss all of them in a rather short document. This has made the manuscript overloaded with data and left many discussed questions poorly resolved and/or presented in a contradictory manner. Just as an example, the distribution of oxygen is claimed to be linear versus sigma-t and it is suggested as a proof of no production and consumption of oxygen in this layer. This assumption is later used to estimate the distribution of oxygen in the pycnocline from the numerically simulated thermohaline fields. Yet, production of oxygen in the upper part of this layer is admitted to reach 75%. Consumption of oxygen should also occur because particulate organic matter is oxidized in the oxycline, nitrate is produced, phosphate and silicate are produced, etc., but all these oxygen consuming processes occur above the suboxic zone. Different dynamics of the upper and lower boundary of the suboxic zone has been previously recognized as an evidence of different processes governing the oxygen and sulfide distribution. This sort of problems is true for many discussed issues.

Are substantial conclusions reached?

Where it comes to conclusions, some results of the work are exceptionally good and interesting. I personally like very much the results of analyses of the importance of diapycnal versus isopycnal mixing. But wording is inappropriate. As few examples, conclusions of this kind "we gave some new insights on the oxygen variability" (what are these insights?), "The temporal evolution of the vertical distribution of oxygen is very different in the coastal and open ocean" (what is the scale of these variations? Is the depth or density scale considered here? What is "very different"?), "The upper boundary of suboxic zone demonstrated much stronger variations in the coastal area than in the deep part of the basin" (What is the used scale? What does mean "coastal" in this case?), etc. make almost impossible to recognize really valuable results of this work. Thus, I believe that conclusions can be substantial, but they must be formulated accordingly.

Are the scientific methods and assumptions valid and clearly outlined?

The applied scientific methods are clearly outlined, but poorly presented. The reason is the same, as it's discussed above. If historical data, AND present-day data from Argo floats, AND 1D and 3D models, AND results of 1D and 3D modeling are attempted to discuss in one manuscript, it becomes a daunting task. I would also strongly recommend to define the used term "suboxic zone", as the authors feel appropriate for their study, but do not use other terms, like "transition zone", etc., for this layer. At the moment, it is a mixture of several poorly defined but different by their nature terms.

When it comes to the suggested assumption of a liner regression for the oxygen distribution versus sigma-t, it is absolutely wrong in my opinion. Even data in Fig. 3 reveals that this regression is not linear. Any statistical data are not suggested. This assumption of a linear regression does not fit all well known to the authors of this manuscript data on oxygen consumption above the suboxic zone and sulfide production in the anoxic water column. Besides, I would appreciate the authors could demonstrate, where McDougall (1984), as the authors referenced this publication, assumed that this sort of linear regression would mean no production and/or consumption.

Are the results sufficient to support the interpretations and conclusions?

I do not think that the suggested results in their presented volume are sufficient. Yet, I would not recommend an increase in the suggested results. I am absolutely sure the authors have such results, but this would overload the manuscript even further. I would



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recommend presenting models and their coupling in another manuscript. I would also recommend presenting results of analysis of historical data in a separate manuscript. This manuscript, if limited to results of modeling and their analysis, would result in the extremely valuable publication on seasonal variations in the distribution of oxygen and the role of dyapycnal mixing in these variations. Yet, this is my personal opinion and the authors are free to make their choice, of course.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

This description is definitely not sufficient for the above discussed reasons. Thus, for example, the only flow-chart (Fig. 4), an equation for the oxygen flux at the sea surface (Appendix A), and an equation for non-conservative substance (equation 1) are suggested in the manuscript. This data is not enough to allow any actual reproduction of the results. The important references by the authors have been suggested, of course. This makes the suggested information both incomplete and redundant. The suggested information, by the way, has never been discussed in detail and it is practically impossible for the limits of the manuscript.

Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

The authors clearly indicate their own new/original contribution, but some important publications on, for example, the oxygen dynamics and thermohaline variations are missed.

Does the title clearly reflect the contents of the paper?

I would recommend a shorter version of the title. For example, "Temporal and spatial variability in the oxygen and sulfide distribution in the Black Sea". If the authors agree to limit their manuscript to modeling, then a possible title: "Modeling temporal and spatial variability in the oxygen and sulfide distribution in the Black Sea waters" or



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"About isopycnal and diapycnal mixing in temporal and spatial variability in the oxygen and sulfide distribution in the Black Sea waters". But this is up to the authors. Yet, biogeochemistry is poorly discussed to announce it in the title.

Does the abstract provide a concise and complete summary?

Yes, it does, but wording must be improved.

Is the overall presentation well structured and clear?

I cannot agree with this. As few examples, the authors discuss general features of the distribution of oxygen and sulfide in section "Historical observations", and they spend a large part of this section for their assumption of a linear regression for the distribution of oxygen versus sigma-t, but the authors have claimed to "describe the temporal and spatial variability of oxygen as seen in historical data", which has never happened. I do not see discussion of any biogeochemistry in section "The Black Sea biogeochemistry as seen in isopycnic coordinates", and this section is absurdly limited (about 30 lines) for this manuscript.

Is the language fluent and precise?

No, it is not. It is extremely important for the authors to dramatically improve the language. Not mentioning typical mistakes with articles, wording, and used verb tenses (English is not my first language too), it is hardly possible to understand the suggested explanations in some cases. As very few examples, "Said in other words, the compromise proposed here neglects the deep-sea processes", "enabled the oxygen to penetrating deeper", "not trivially linked to the ones in the oxygen and sulphide dominated layers", "in particular the ones taking place", etc.

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes, they are correctly defined and used.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

First of all, there are about 70 panels in 16 figures in the main part of the manuscript and 9 more panels in two figures in annexes. That's definitely too many for a paper. Some figures are hard to see in details because, for example, 16 (!) panels are plotted in one figure. Some other figures, like Fig. 4, or tables, like Table A1, for example, can be eliminated because they are reproduced from previous publications by the authors and they have never been effectively discussed in this manuscript.

Are the number and quality of references appropriate?

Some important publications for the oxygen dynamics in the Black Sea have been missed to either confirm or argue some ideas and results in this manuscript. Yet, it's up to the authors. Another issue is important. The previous publications by the authors are important. I do not have any doubts about that. But the number of these publications is about 38% of the total number of publications. As far as I know, the recommended value is under 20%.

Is the amount and quality of supplementary material appropriate?

It's hardly appropriate. It's not sufficient in the present form. I would suggest either eliminate this supplementary material and refer to the previous publications by the authors or increase this material. It looks also good to remove data on the models from the main part of the text and place it to annexes.

Finally, I want to point that data of this manuscript is of extreme scientific importance and I fully support the intention of the authors to publish it. Yet, the manuscript should be improved in its form. Thus, almost all my comments and suggestions, except for the assumption of a linear regression of the oxygen distribution versus sigma-t, are addressed to the form of this manuscript, rather than the results.

Interactive comment on Biogeosciences Discuss., 11, 281, 2014.