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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.

E. V. Stanev et al.

emil.stanev@hzg.de

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Answers to reviewers comments on "The Black Sea biogeochemistry: focus on temporal and spatial variability of oxygen".

Referee #1

We are very grateful to referee #1 (S.K. Konovalov) for his appreciation of our research results and their novel character. We appreciate his constructive suggestions and comments. We almost fully addressed his (in the following in italic) comments and present below our point-by-point answers and explanations.

General comments: As far as the suggestions what to keep in and what to remove from the revised manuscript is concerned, we partially made our decision based on

the statement of S. Konovalov that "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." Because the reviewer considers that this is "relevant scientific question within the scope of BG" we keep this issue in the revised manuscript. At the same time we restructured the present submission (see in the following how).

The major drawback of this manuscript is that the authors have raised too many questions . . . and/or presented in a contradictory manner. Authors: We restructured the manuscript and tried to identify in a clear way the major issues addressed in the paper. We also clarified in the resubmission the issues mentioned by the reviewer (see below the answers to specific questions).

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? Authors: We consider this as the major criticism, which we accept. The reviewer is right. We accept that the presentation of this aspect was not good. The corresponding explanations are given in the revised manuscript. We bring to the attention of the reviewer that the dependence shown in this plot was just a result of some observations. In the revised manuscript we produce new analyses using big data set (Argo floats), which quantify the relationship between isopycnal mixing and the rest, that is the consumption of

DO for POM (and DOM) mineralization, respiration of heterotrophs and for nitrification. In the revised manuscript we modify the text of this figure description. We present in the revised manuscript quantification of the deviations from the linear (in density coordinates) distribution in different locations, which actually reveals the rate of isopycal diffusion. These analyses are very important to evaluate the exchange between coastal and open-ocean.

. . .As few examples, conclusions of this kind "we gave some new insights on the oxygen variability" (what are these insights?), Authors: This has been reformulated.

"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"?), Authors: More detail about the scales is given in the revised manuscript.

"The upper boundary of suboxic zone demonstrated much stronger variations in the coastal are than in the deep part of the basin" (What is the used scale? Authors: More detail about the scales is given in the revised manuscript.

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. Authors: In the re-submission we give more focused specification on what we understand under coastal. Wording has also been changed and conclusions have also been re-formulated.

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. Authors: We improved the presentation of methods (basically the analysis on different data and explanations based on them). As we mentioned in the revised paper, this work integrates observation and modelling.

Our opinion is that there are many papers focused either on modelling or observations, thus the challenge here is to address both of them in one paper. As a response to this comment we reduced part of the material which can be found in other papers (1D modelling) providing appropriate citations.

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. Authors: Terms have been clearly specified in the revised manuscript. We use terms from different fields, for example surface layer or upper mixed layer or euphotic layer are different terms for approximately the same layer. The same way the terms "redox-layer, suboxic layer, nepheloid redox layer, turbidity layer correspond to the same depths. But, a strictly defined suboxic layer is probably worth that redox layer, since it considers processes started above the suboxic layer (i.e. denitrification, that occur at DO>10 uM) and sulfate reduction, that has maximum in the suldfidic (or anoxic) layer. There is a strong opinion, that the term suboxic should not be used at all (Canfield). Again the usage of a term depends on the context. In the revised version we clarify the use of terms.

When it comes to the 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. Authors: We address this comment in a new sub-section providing quantitative estimates for linearity (and deviation from linearity based on Argo observations). Also see an answer above.

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. Authors: We have already commented this above admitting that this is a very good and relevant point of reviewer and address it in detail in the revised manuscript, acknowledging the suggestions of S. Konovalov.

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. Authors: We gave in the revised manuscript more appropriate reference to this issue.

I would 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. Authors: As we explained above, the basic issue here is to present data AND modelling together. In future publications we will focus on specific details.

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. Authors: In the revised manuscript some parts have been omitted and necessary citations given in order to address the comment about “incomplete and redundant” information. The models used here were described in details in many publication that we can just refer to. This is a general way how the modeling results are usually presented nowadays.

. . . some important publications on, for example, the oxygen dynamics and thermohaline variations are missed. Authors: we included some relevant titles.

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 "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. Authors: we changed the title as: "Temporal and spatial variability of oxygen and sulfide in the Black Sea. Observations and modelling"

Does the abstract provide a concise and complete summary? Yes, it does, but wording must be improved. Authors: we improved the wording.

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. Authors: This part has been restructured and new quantitative information added.

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. Authors: The title of this section has been changed and the section itself restructured.

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. Authors: These specific formulations have been improved along with other language-changes.

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. Authors: We addressed this comment as suggested and reduce part of illustrations.

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%. Authors: We removed some of our publications from the reference list,

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. Authors: We assume that referee understands Annexes as supplementary material. If so we agree with him and reduced Annexes.

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