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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.

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Referee #2

We are grateful to referee #2 for his/her appreciation that the paper addresses relevant scientific questions within the scope of Biogeosciences and that it presents novel concepts, ideas, tools, or data. We appreciate also his/her constructive suggestions and comments. We almost fully addressed in the revised manuscript these comments (in the following point-by-point answers in italic)..

General Comments: The biogeochemical model involves a complex redox dynamics that is in fact far more complex than necessary for the purpose of the present study. On the contrary, the model possesses a very crude upper layer biological structure

that is in fact linked to the oxygen dynamics more tightly than the processes taking place at the oxic-anoxic interface zone. This model was originally designed by one of the co-authors (E. Yakushev) to study the complex redox dynamics of the Black Sea from a biogeochemical perspective. The alternative more simplified models are also available with more simplified representation of the redox layer biogeochemical processes. The present model crudely represents the impacts of biological processes (e.g. primary production, remineralization, excretion, etc) on the oxygen structure at depths above the suboxic zone, whereas the complex redox dynamics within the suboxic and anoxic layers is redundant to study oxygen dynamics in the, by definition, oxygen deficient zone. The use of this type complex biogeochemical model structure may be necessary and justified for studying other aspects of the biogeochemistry but not for the oxygen. Authors: We disagree, that the complex redox dynamics is not needed for the oxygen modeling above the suboxic zone. The reduced and intermediate species of metals (Mn(II), Mn(III), Fe(II), S2O3, SO) are an important sink of DO in all the suboxic zone. If these processes would not be included, the model would produce increased values at these depths, and therefore, deeper positions of the oxygen isolines than they should be. In Yakushev et al (2007) the consumption of oxygen was analyzed, and was shown that in low oxygen conditions (DO<30 uM) 50% of DO is consumed for the processes connected with redox processes. This effect is in particular included in the Baltic Sea modeling (http://www.balticnest.org/balticnest/research/publications/publications/baltsemamarinemode and the North Sea (ECOSMO model, Corinna Schrumm, p.c.)

A signal from this oxygen consumption affects (due to a dominant isopicnal mixing, as it was shown in this paper) the upper layers. The ecosystem parameterization in ROLM does not include all details of the ecosystem functioning, but nevertheless allows parameterize the main features of the DO fate in the upper layer.

We admit that the purpose of the present study was not enough explained, in particular as far the need for using this specific model is justified. We explain clearly in the revised manuscript why exactly we use the chosen model. We stress in the revised manuscript that in this study we do address the complex redox dynamics of the Black Sea from a biogeochemical perspective. To this we want to add that that we addressed in the conclusions the comment of the referee saying that the success of using simpler models needs to be checked. This could present an issue in future studies.

Indeed the authors have ended up focussing on mainly the physical processes for explaing the oxygen structure within mostly upper parts of the water column away from the oxicanoxic interface zone. Authors: We agree and say that in the paper we want to understand the role of physical factors. This is the main focus of the paper and we put it clear in the revised manuscript. However, we do not agree that we "look" mostly upper parts of the water column away from the oxicanoxic interface zone. Please see Fig. 3. We stress that this is the major interface in which we address in the present study. See an answer above also.

Indeed, they did not provide any justification why they have chosen such a complex biogeochemical model or, in other words, why such a complex biogeochemical model was necessary to study the oxygen dynamics. Authors: We mention in the revised manuscript that the choice reflects the major interest in the present paper, which is the dynamics of the oxic-anoxic interface zone.

To my opinion, excluding the manganese model and parameterizing it in much simpler terms would not alter the results described in the manuscript. This is an important issue because the coupled physical-biogeochemical models can not practically accommodate such a complex biogeochemical dynamics for practical reasons in long-term decadal simulations. It would be good to know how much simplifications can be appropriately done without scarifying much from reality. I suggest authors to include a discussion section on these issues.

Authors: See above. We added the justification. As said above we address this issue in the conclusions. However, we stress that in this paper we do not aim to make model

development, neither to make model-model inter-comparison. We have addressed large number of issues, and as the first reviewer suggested, we reduced part of model aspects in the revised manuscript. Important to bring to the attention of the referee is that we use available models with proven qualities, which are checked in previous publications with the aim to study oxygen dynamics in the transition zone. We admit that there are other developments, which are very appropriate for the shelf regions, but our major focus is in the deep part of Black Sea. And this is what makes our work different from many nice modelling works on other aspects of biogeochemistry of Black Sea.

The second issue is the focus of the study. There is no specific problem to be solved and/or a hypothesis to be tested. I find the manuscript too broad and many issues are touched up on briefly without providing details on the specific mechanisms responsible for them. In fact, may of the issues presented have already been known from the previous studies. The manuscript may be considered as an overview paper linking many different aspects of the Black Sea oxygen characteristics to the physical characteristics of the system. To my opinion, the most interesting part of the manuscript is the section 6.2.2 and Fig. 16 that could indeed form a novel scientific research paper by itself and would provide a nice contribution to the scientific understanding of the Black Sea hydrochemistry because this particular subject has not been elaborated in sufficient details up to now to my knowledge. Authors: We agree with the reviewer. However the way how to generate and explain the information in Fig. 6 needs to be described (and this is what the paper is about). Without this, the results presented would not be easily justifiable and understandable. Following the recommendation of the reviewer we structured the re-submission in order to better elucidate the focus of this study.

Issues like impacts of the rim current meanders and mesoscale eddies together with the contribution of upper-layer biogeochemical processes on the local oxygen dynamics are highly novel issues for a broader oceanograpgic community. Unfortunately, they are presented only broadly in the manuscript. They need to be elaborated in suffient detail and form a main focus of the text while some other sections may be shorthed or taken out completely if the manuscript will be decided to appear in the journal. Authors: We are grateful for this suggestion and restructured the manuscript as proposed by the referee, removing part of information and adding new quantitative details.

The title of the manuscript is too ambitious to me. It gives a wrong impression and has nothing to do with the biogeochemistry of the Black Sea. The manuscript simply deals with how the oxygen structure is regulated/controlled by the physical processes. Authors: We changed the title accordingly (and followed the suggestion of the first referee).

Albeit all these deficiencies, I find the manuscript as a useful contribution to the existing Black Sea literature, and it is worth publishing in Biogeosciences provided that my comments are incorporated in the revised manuscript. Authors: We address all comment of the referee in the revised manuscript.

Presentation Quality: Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)? Yes, but to many irrelevant details are present. Authors: Some details have been removed from the revised manuscript.

Are substantial conclusions reached? No. Authors: In the revised manuscript we make clear which results are substantial.

Are the scientific methods and assumptions valid and clearly outlined? No. Authors: We do not agree that they are not valid. However, we outlined them more carefully in the revised manuscript.

Are the results sufficient to support the interpretations and conclusions? No. Authors: We added new quantitative results, which also address the comments of first referee.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? No. Authors:

We mentioned in the revised manuscript that the presentation aims at reproduction of results and we give all needed parameters, references to relevant papers where further parameters ale listed, as well as to the sources of boundary conditions (including atmospheric forcing).

Does the title clearly reflect the contents of the paper? No. Authors: The title has been changed as"Temporal and spatial variability of oxygen and sulfide in the Black Sea. Obsevations and modelling"

Does the abstract provide a concise and complete summary? No. Authors: Abstract has been reformulated in some parts.

Is the overall presentation well structured and clear? No. Authors: Better structure is now put in place following the referee's comments.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes. Authors: we followed this recommendation when preparing the revised manuscript.

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