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Biogeosciences Discussions

Interactive comment on "A new estimate of ocean oxygen utilization points to a reduced rate of respiration in the ocean interior" *by* O. Duteil et al.

Anonymous Referee #2

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Summary: Duteil and co-authors develop a new approach to estimate oxygen utilization rates in the ocean, the so-called Evaluated Oxygen Utilization (EOU). The new approach tries to account for the atmosphere-ocean oxygen disequilibrium in subduction regions. By using an modeled idealized preformed oxygen tracer, the authors show that the new approach outperforms the classical approach based on apparent oxygen utilization in six different ocean models. By applying the method to observational-based data, the authors suggest that the biological oxygen consumption rate is 25 percent lower than derived from AOU-based estimates.

Evaluation: It is well known that the oxygen concentration at the surface is not exactly at saturation and that AOU overestimates oxygen utilization rates. So far, to my knowledge, a 'simple' method to quantify the impact of the undersaturation on oxygen

utilization rates is missing. Furthermore, no multi-model intercomparison of oxygen using an idealized preformed oxygen tracer has been performed. This paper addresses a clear gap in our understanding of the respiration in the open ocean, and therefore represent a welcomed and important contribution to the field.

The paper is overall well written, and clearly organized. It addresses an important topic, and the method and results are of interest to a wider community.

Recommendation: I recommend acceptance of this manuscript after moderate revisions. I particularly recommend that the authors extend their introduction and discussion section to put the new findings into a broader context. Furthermore, the manuscript lacks of important details that have to be addressed before publication.

Major comments 1. Although I appreciate the effort to keep the paper short and dense, I recommend to extend the introduction and discussion section to put the new findings into context. Why is it important to have adequate oxygen consumption rates? What are the implications of a 25 percent lower OUR? Does the study change our understanding of the respiration processes in the open ocean? Instead of 'just' introducing the new method, discussing the implications of the new OUR estimate would be of interest to a broad audience. 2. The result section ('Computations of AOU, EOU and TOU') lacks of details and the description of the figures could be much sharper. There is enough room in a journal such as Biogeosciences to discuss the results in more detail (see specific comments below). 3. While I appreciate the effort to come up with a new approach to estimate the oxygen utilization rate, I am not really convinced about the robustness of the results when applying to observational-based data. As pointed out by the anonymous reviewer #1, the uncertainties in the observational-based EOU estimates have to be addressed in much more detail. Even if the EOU method works well in a model framework, it does not mean that it also works in the real world. 4. The models used in this study are very coarse, and more up-to-date CMIP5-type (ocean) models are available. While I know, that the CMIP5 model output does not include an idealized preformed oxygen tracer, it might be helpful to discuss in more detail the

limitations of the six ocean models used in this study. How do they compare with other models (e.g. used in Cocco et al. 2013)? The authors claim that the six models realistically represent the combined effects of circulation and biology (p. 2250 l. 4). Having a closer look at Fig. 1, you need to convince me. What is the most critical thing to get right in the models, to successfully represent OUR with EOU? Why are the EOU estimates so different in the different models when comparing with TOU?

Specific comments: p.2246 l. 7. 'twice as well' please specify if globally or regionally. p.2246 l. 13. 'respiration is a key biological process in the ocean' . Please extend the introduction to put your study into a broader context. p. 2246. l. 18-22. Be more specific. What about supersaturation? Vast areas in mid-to-low latitudes are slightly supersaturated (see Fig. 3.1.1 in Sarmiento and Gruber 2006). p. 2246 l. 19. What sort of physical and chemical processes? p. 2246. l. 26. What was the motivation to use six different ocean models? Please specify. p. 2248 l. 12 Why do the authors take annual-mean model output? I assume that monthly output should be available for at least a subset of models. p. 2249. l. 7. How large is the drift in the control simulations of the forward models? p. 2249. l. 10. Why not using the improved O2 data set from Bianchi et al. 2012 (GBC)? p. 2249. l. 22. 'sluggish' be more specific. p. 2250. l. 1-5. What about the deficiencies in the models? Please specify them and discuss the implications for the EOU estimates. p. 2250 l 14-15 'O2 pre is always less than O2 sat ' What about the negative values in the low latitudes? p. 2250. l. 16. Why does om1p7-BLINGv0 poorly represent AOU in the Southern Ocean and the deep Pacific Ocean. p. 2250 l. 15 'Maximum AOU-TOU differences ...' Do you mean along the transects investigated in Fig. 2? p. 2250 l.18. Be more specific where Ito et al. (2004) found their maximum O2 disequilibrium. p. 2252 l. 12-20. This can clearly be tested by using monthly model output instead of annual mean output. p. 2254. l. 12: How sensitive are the results to the choice of the depth horizon? Give some uncertainty estimates. p. 2256. l. Do the results shown in Fig B1 represent single year values? If yes, the results may just show one single large convection event in the Southern Ocean. p. 2265. Fig 4a (right) Please adjust color bar to make differences visible.

Interactive comment on Biogeosciences Discuss., 10, 2245, 2013.