Biogeosciences Discuss., 10, C851–C854, 2013
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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 #1**

Received and published: 8 April 2013

The manuscript "A new estimate of ocean oxygen utilization points to a reduced rate if respiration in the ocean interior" presents a method (the EOU method) of calculating the integrated oxygen utilization at any point in the interior ocean. Because the new method accounts for oxygen undersaturation at the ocean surface, it predicts about 25% weaker biological oxygen utilization than the AOU method.

That AOU overpredicts oxygen utilization due to surface O2 undersaturation is well known. The value of the present manuscript is that the authors present a method for quantifying this undersaturation, thereby deriving a less biased estimate of oxygen utilization, as supported by application of the EOU method to model output. However, this value is muted by the fact that the authors do not adequately address the limitations, uncertainties, and potential biases of their admittedly ad-hoc correction method. The



method will be most useful applied to observations, and this is where the authors need to do a much better job of addressing the uncertainties. Modelers are unlikely to use this method to determine oxygen utilization, as running a preformed oxygen tracer is straightforward and much more accurate.

I recommend that the manuscript be revised to better address uncertainties in the observationally-estimated EOU. Specifically, the following sources of error should be quantified and explicitly discussed in the revised manuscript: 1. Sensitivity of oxygen utilization predicted by the EOU method to the number of isopycnal layers. This is addressed for the models in Figure A1, but also needs to be addressed for the observations. Are the observational EOU estimates highly sensitive to the number of isopycnal layers (like UVIC or BLING models) or insensitive (like MIT or CSIRO models)? What mechanism determines the sensitivity of the EOU estimate to the number of isopycnal layers? 2. The effects of spatially and temporally variable surface saturations. The densest waters in the ocean outcrop in both the North Atlantic and the Southern Ocean, where surface saturations are very different. Also, surface saturations and water-mass formation rates vary seasonally. So the authors need to discuss how the assumption of a uniform (spatial and temporal average) surface saturation for the deepest waters affects the oxygen utilization inferred by the EOU method. The spatial differences in saturation are shown in Figure B1, but this is a significant source of error/bias that needs to be quantified and discussed in the main text.

Specific line items:

Page 2248, Line 15: need to be more clear about what is meant by "TS properties"

Page 2249, Line 7: 2000 years is not long enough for the deep ocean (esp. Pacific) to reach equilibrium with the surface forcing. What are the model drifts in O2?

Page 2249, Line 19: The statement "The deep Pacific Ocean is solely filled by CDW" is not true. See e.g. Gebbie and Huybers, 2010; DeVries and Primeau, 2011.

Page 2249, Line 21: intermediate depth -> be more specific

Page 2249, Line 22-23: "sluggish circulation" is debatable. Equatorial current systems are very strong.

Page 2253, Lines 12-15: Need to include uncertainty on EOU estimate of reg/tot nutrients. Examples of sources of uncertainty include number of isopycnal layers used to compute the EOU, variability in surface saturation, and ratio of O2:P. What ratio of O2:P is assumed?

Page 2255: Cut out the conceptual script as it is not needed. The calculation is straightforward.

Page 2254-5, Lines 24-2: This issue needs to be better addressed and contribution to EOU uncertainty quantified.

Page 2256, Lines 17-25: This issue needs to be better addressed and contribution to EOU uncertainty quantified.

Page 2256, Line 23: Are the values of diapycnal diffusivity in the interior ocean different in the models? If so, what are the values? What are the overturning strengths in the models?

Table 2: Should add uncertainty to WOA EOU estimates (115.5 +- ? mmol/mˆ3 and 0.30 +- ?) due to variability in surface saturation, # of isopycnal layers used, etc.

Figure 2: The authors could do a better job of addressing why the EOU-TOU differences vary so widely among the models. What do the discrepancies tell us about the circulations of the various models? What can they tell us about potential biases in observational EOU estimates?

Figure 4a, right panel: The colors seem to jump from pink (10-20) to orange (30-40) with little splotches of red along the 27.5 contour. Why?

Minor corrections:



Page 2246, Line 10: spurious "by"

Page 2248, Line 24: four -> five

Page 2253, Line 27: then -> than

Page 2254, Line 11: discretisize sp.

Page 2256, Line 11: delete "rate"; percents -> percent

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