Friday, November 15, 2013

Dear Dr. Suzuki,

Attached is a revised version of our manuscript, entitled: “The effect of vertical turbulent mixing on gross O_2 production assessments by the triple isotopic composition of dissolved O_2". (bg-2013-412)

We thank the assigned reviewers for taking the time to carefully read our manuscript. Their remarks and advices were very useful and helped strengthen our arguments and improve their presentation.

Following the reviewers‘ comments, we tested the sensitivity of the GOP correction for turbulent flux of O_2 from the thermocline to the depth of the point representing the thermocline, the analytical error associated with $^{17}\Delta$ measurements, the eddy-diffusivity coefficient and the mixed-layer depth. The results of these tests are presented in a new section, illustrated by a new figure, and are discussed throughout the discussion section. In the comparison to previously published GOP rates, we followed reviewer’s #2 comment and used the gas-exchange parameterization of Sweeney et al. (2007) rather than Wanninkhof (1992).

Other than that, we have related to each of the reviewers’ comments. In the next pages, we provide detailed description of the changes we made in the text.

Best regards,

Eyal Wurgaft

Anonymous Referee #1

Estimation of marine primary production using triple isotopic composition of dissolved oxygen, which was founded in 2000, has been activated by numerous publications after 2010. Through its development, one significant issue for interactions with subsurface layer beneath the mixed layer has been raised. This study by Wurgaft, Shamir,and Angert aims to evaluate the diapycnal oxygen transfer between mixed layer and underlying waters by vertical one-dimension model including eddy diffusion process.

The paper was written clearly and well organized; the argument is simple but important. Introductory section seems almost perfect because the objective and motivation of this study are clear and fashionable. I agree with future perspectives shown by authors in the discussion section. Basically, I think this paper is worth-publishing in Biogeosciences.

However, I also think authors should pay some more efforts on their model calculation. As authors themselves pointed out, the simulated results appears to be highly dependent on the choice of eddy diffusivity, nonetheless they applied only one arbitrary value (along with one extreme value). As a consequence, it is difficult to find any persuasiveness from section 3 and 4. I strongly recommend authors to do sensitivity test by using various eddy diffusivities. An additional sensitivity test are also recommendable to change "deep" points. These results may make readers understand how significant these parameters are.

We thank the reviewer for this important remark. We added a new section to the manuscript, accompanied by a new figure, in which we describe the sensitivity of the GOP correction to the eddy-diffusivity, to the depth of the assigned "deep" point (for clarity, we now relate to it as the “thermocline” point), to the analytical error associated with $^{17}\Delta$ measurements, and to the mixed-layer depth.

I have another two questions to avoid confusion.

Q1. Authors prescribed a depth of single layer to 10 m in both Section 3 and supplementary document. But in the Table 1 and 2, ones digits of all depths were 5. Why?

More specifically, authors prescribed an uppermost layer to 10 m, whereas the thinnest mixed layer in Table 2 was 5 m. Why?

This was a technical mistake, stemming from the way in which the model is plotting the results. We corrected the tables to match the description of the model.

Q2. Authors let the uppermost layer been in equilibrium with atmospheric oxygen both for concentration and isotopes. How about mixed layer? Is it always equilibrated with atmosphere both for concentration and isotopes?

The isotopic composition and O_2 concentrations in the mixed-layer are controlled by a combination of several fluxes (GOP, respiration, turbulent mixing with the thermocline and turbulent mixing with the uppermost layer) and their associated isotopic fractionations. Having realized that this point was not clearly presented, we added a short explanation about the dynamics of O_2 isotopolouges in the mixed-layer in P6. L18-21.

Please refer following minor comments as well.

P14241L15-16: equilibrium concentration of O2 with atmosphere

Done.

P14241L16: equilibrium 17D with atmospheric O2

Done.

P14242L25: Replace "takes place" to "dominates"

Done.

P14244L19: Remove comma between "column" and "represented"

Done.

P14244L21: Replace "so that [O2] would remain fully mixed" to "so as to let oxygen in the ML fully mixed"

Done.

P14245L25: Authors never define the correspondence of model days with real months.

The point was clarified in the model description, in P7. L4 and P7.L17

Anonymous Referee #2

Received and published: 24 October 2013

Authors of the manuscript "The effect of vertical turbulent mixing on gross O2 production assessments by the triple isotopic composition of dissolved O2" explore the influence of turbulent mixing between the mixed layer and sub-mixed layer in interpreting the triple oxygen isotopic composition of dissolved oxygen as a tracer of gross photosynthetic Oxygen production. The study employs a 1D model.

The usefulness of the proposed correction scheme is limited by several factors pointed out by the authors including (1) constant mixed layer depth (2) ambiguity in defining the actual gradient at the base of the mixed layer (i.e. how to define the deep reference point) (3) Assuming a constant and somewhat arbitrary constant for vertical diffusivity. Because a number of the model parameters are either difficult to determine in the field, or may vary significantly in space and time, it would be useful for the authors to more rigorously explore the sensitivity of their estimates to variations in a number of parameters, including the diffusive mixing constant, as well as the mixed layer depth, h, which is set at a constant value of 35m. If h is set at a shallower constant value, e.g. 20m, the vertical turbulent mixing effect would be even larger, and vice-versa. It would be nice to see some sort of uncertainty analysis to give some confidence intervals as to how well the turbulent mixing bias can be estimated given a range of values for input parameters.

In general, while limited in applicability, I think the authors investigate an important process in a tracer system that is becoming more and more widespread in its application. Consideration of the issues raised by the authors is certainly worthwhile. The authors introduce these potential issues in a clear and thoughtful manner. My primary suggestion is that a level of depth should be added to their analysis and interpretation.

We thank the reviewer for this important remark. We added a new section to the manuscript, accompanied by a new figure, in which we describe the sensitivity of the turbulence correction to the parameters listed by the reviewer, as well as to the analytical error associated with $^{17}\Delta$ measurements. The reviewer's point regarding the importance of the effect of turbulence as a function of the mixed-layer depth was very much correct. It is now discussed in P10 L3-7.

Additional concerns:
The model is initialized with a vertical profile at the start of the year. For much of the ocean this is unrealistic (i.e. it may be reasonable for BATS, but not for HOT, and not for the Equatorial Pacific, or most of the ocean mixed layer that never mixes down to 300m). Is Oxygen reasonably simulated by the model? What is the impact of mixing with undersaturated water from below? The 1D model does not seem to include this.

The modeled O_2 concentrations were fairly realistic, since our choice of GOP, gas-exchange rate and respiration were based on typical values from BATS and HOT. The 1D model, however, was not designed to preform accurate simulations of the annual O_2 cycle, but rather to constitute a numerical framework to examine of the effect of turbulent flux on $G^{17}OP$ estimations. In this sense, the numeric model was merely a tool we used to investigate certain aspect of our analytical model (i.e. Eq.4), and therefore, we did not discuss the modeled O_2 concentrations in the manuscript. It should be noted that ^{17}O-excess is essentially independent of O_2 concentrations (which is the reason it can be used to estimate gross production).

The more recent gas flux parameterizations (e.g. Ho et al 2006, Sweeney et al. 2007) are likely to be more accurate than the Wanninkhof 1992 parameterization, as W92 depended on early estimates of 14C inventory for the ocean that were about 25% too high. (see Wanninkhof 2009, Annual Review of Marine Science). Either Ho or Sweeney would be a better choice for the default gas exchange for modeling purposes.

We thank the reviewer for this suggestion. We re-calculated the $G^{17}OP_C$ using Sweeney et al. 2007 gas-exchange parameterization.

Typographical suggestions:

p14240 l16: factor → process
Done.

p14241 l6: âLURref are the → *R are the (remove ref subscript)
The “ref” subscript is assigned so that readers who are not familiar with the δ notation could easily understand its meaning. We feel that for the sake of clarity, it should remain unchanged, (It was somewhat hard to understand the reviewer’s suggestion here, since the text was distorted. If we misinterpreted his/hers intention, please let us know).

p14241 l22: their equations were not approximated → their equations avoided a number of numerical approximations (They still include some approximations and assumptions)
Good point. Done.

p14241 l4: are small→are small for typical open ocean conditions
Done.

p14242 l8: a balance→a steady-state balance
Done.

p14242 l17-19: net productivity→ net primary productivity and N14CP→NPP(14C) or N14CPP (so that it is not confused with NCP, which is commonly used to refer to Net Community Production, a very different quantity that 14C NPP.
Done.

p14242 l2: go back to→revisit
Done.

p14242 l21: h→h (italicize)
Done.

p14246 l17: Wannikhof→Wanninkhof
Done.

p14246 l21: Sweeny→Sweeney
Done.