

Interactive comment on “Technical note: The effect of vertical turbulent mixing on gross O₂ production assessments by the triple isotopic composition of dissolved O₂” by E. Wurgaft et al.

Anonymous Referee #2

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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.

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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, κ , 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.

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 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.

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Typographical suggestions:

p14240 l16: factor → process

p14241 l6: $\hat{U}R_{ref}$ are the → $*R$ are the (remove ref subscript)

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

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

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

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 than 14C NPP).

p14242 l2: go back to → revisit

p14242 l21: h → h (italicize)

p14246 l17: Wannikhof → Wanninkhof

p14246 l21: Sweeny → Sweeney

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