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Biogeosciences Discussions

Interactive comment on "Oxygen, carbon, and nutrients in the oligotrophic eastern subtropical North Atlantic" *by* P. Kähler et al.

Anonymous Referee #1

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General Comments:

This paper reports data collected along a short meridional section in the eastern North Atlantic subtropical gyre. These are used to assess apparent imbalances between OUR in the subsurface and new/export production from the surface. The idea is that DOC export will contribute to OUR, making OUR larger than nitrate consumption alone would suggest, and that utilization of N held in DON is an additional source of new N (along with N2 fixation).

I could not agree with the author's interpretations of gradients in variables on several isopycnal surfaces (Figs. 2-6). The authors reported gradients where my eye did not necessarily find one. For example, in determining AOU vs space gradients they included data from the surface mixed layer. I think this inclusion is inappropriate. The

AOU gradients are much different without those surface values. Also, the AOU values (and other variables) do not show consistent gradients. At some points they increase along an isopycnal while elsewhere they decrease. Yet the regressions applied are linear through these rising and decreasing gradients, ignoring the fact that the gradients are not uniform in space. Similarly, the TOC data regressions included surface values. If those are removed from consideration, then the gradients largely disappear. Surface values should not, in general, be used when consider changes within the thermocline.

The authors have too few data to conduct this analysis. In the lightest isopycnals, they have only half a dozen data points, and these can be inconsistent in gradients. On heavier isopycnals, the gradients are weak. Just as importantly as too few data, the authors do not have the right data. They need age tracers for the water masses.

Given the great limitations in the interpretations given in Section 3.1, the foundation is too weak to support subsequent interpretations in the paper. The authors are reaching too far with too few data.

Specific Comments:

Page/line

8925/1-7 – It is misleading to state that the "biotic contribution to the air/sea flux of CO2 in the oligotrophic subtropical gyres.." was the focus of the controversy referred to in the Introduction. The controversy was whether the surface ocean was net heterotrophic or net autotrophic. The related debate was not explicitly connected to the issue of air/sea exchange. The papers referenced generally did not report delta pCO2 values, so they could not directly address the issue of CO2 flux to/from the atmosphere. One can infer something about effects of metabolism on air/sea flux, but it wasn't the focus.

8925/10-13 – "... this would imply that biota add CO2 to the atmosphere in the oligotrophic regions..". No, it doesn't imply that. It would make such an exchange more favorable, but CO2 is added to the atmosphere only if the delta pCO2 favors transfer. A net heterotrophic system does not necessarily result in a positive sea to air flux. I'm not comfortable with the authors so strongly tying metabolic balance to air/sea exchange, given the absence of pCO2 data in the papers that address the metabolic issue.

8926/7-8 - I'm not sure that one can get "rates" from "differences in stocks…"in the direction of water transport". This sentence needs to be clarified. Changes in stocks with aging of water masses may be what is meant.

8930/20-24 – The fact that samples were lost is irrelevant. The readers need to know which reported data were collected in 2001 and which in 2002.

8931/9-20 – I don't understand this section describing 'water transport rates'. What does it mean to compare states rather than rates? How is a comparison done with Jenkins if no age tracers were included in the measurement suite?

8931/21-22 - In reading this, I don't understand how to get TOC accumulation rates from velocity fields. I do understand once I read the Results, but I should understand after having read the Methods. These 'methods' descriptions for getting at rates need to be improved.

8932/21-23 - It is not valid to include in the regressions the data from the surface mixed layer. This concern is particularly acute for the lowest density waters, where there is almost no gradient if the surface layer data points are excluded (note that if the surface AOU's are removed from fig 2, then there is little gradient). The fact that AOU is negative in those two points tells me that the system is not removed from atmospheric influences (bubble injection of O2 is taking place) and so should not be included in the regressions.

8933/section on AOU – a) the higher AOU gradient observed in this work (relative to Jenkins) is probably due to the inclusion of surface data where AOU is negative; this shouldn't be done. B) I'm uncomfortable with the idea that that the authors infer high rates of oxygen consumption by comparison with Jenkins, and that using the result

of the inference leads them to believe that this is evidence for a mismatch between OUR and nitrate supply. This analysis requires age tracers. There needs to more quantification of budgets.

8933/section on TOC – the TOC gradients in the upper two isopycnal layers exist because surface water was included in the regression. It may be okay to assume a one-end member model when working in the thermocline, but I don't believe the authors should include surface waters in such an analysis. If those data points are left out of the regression, then there is essentially no gradient in AOU or TOC, so one cannot infer that TOC drives a large fraction of OUR.

Figure 8 caption is missing symbols. I don't know which data are DOC and which are DON.

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