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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.

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We thank the two reviewers for their constructive criticism which help to improve the quality of our manuscript. Below we provide our detailed responses.

Anonymous Referee #1

General Comments:

Reviewer: 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 N_2 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.

Answer: We are aware that the time elapsed since water parcels left the surface mixed layer (water age) does not increase monotonically towards the south on given isopycnals. We do not need this assumption, however, to make our point, even recognizing

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that to draw a line through data which is not aligned on a trend invites criticism. The point is that on spatial scales considerably bigger than eddy scales (which is the case here, as the stretch of transect considered in our paper extends a couple of hundred kilometers) we can assume with high certainty that water age is higher in the south than in the north on a given isopycnal. Observational evidence supporting this point was presented by e.g. Jenkins 1987. Since the outcrop positions of the two uppermost isopycnals which are those displaying the most marked north-south differences are still in the oligotrophic subtropical gyre we assume that the preformed values of waters were similar irrespective of the exact location of the subduction site. By comparing biogeochemical proxies from the northernmost station with those from 23N we indeed compare biogeochemical proxies of younger and older water parcels even though our transect is not perfectly aligned with water movement and water age may well increase not monotonically from station to station. Note that the three southernmost stations were excluded from the analysis, as we explained in the text. This was guided by a watermass analysis based on temperature, salinity and silicate which showed an abrupt increase of South Atlantic Central Water on $sigma_0$ = 26.62 south of 23N (for details see Dietze et al. 2004).

On average, however, the age will increase towards the south and the comparison of linear trends was chosen as a means of averaging our data. Note that all conclusions drawn in our paper could be obtained as well by just comparing the northernmost stations with data from 23N on respective isopycnals.

Specific comments:

Reviewer: Page/line 8925/1-7 – It is misleading to state that the "biotic contribution to the air/sea flux of CO_2 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 explic-

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itly connected to the issue of air/sea exchange. The papers referenced generally did not report delta pCO_2 values, so they could not directly address the issue of CO_2 flux to/from the atmosphere. One can infer something about effects of metabolism on air/sea flux, but it wasn't the focus.

Answer: We agree with the reviewer in that the papers describing the controversies did not generally refer to CO_2 fluxes or pCO_2. In the first paragraph of the paper we therefore describe which consequences the trophic state of the surface ocean has for the biotic contribution to the air-sea flux of CO_2. The whole introduction is written to show the relatedness of several controversial concepts (i.e. several controversies, of which the one about heterotrophy is but one) about the oligotrophic subtropical ocean. Also the work of Jenkins and Lewis did not make reference to the trophic state of the surface layer, or the consequences on air-sea exchange, but heterotrophy would have been a contribution to the solution of the nutrient-organic matter imbalances of Jenkins and co-workers.

In the revised version we shall make the distinction clearer between the focus of the previous studies and our attempt to put these in a common context in terms of the ocean's carbon balance.

Reviewer: 8925/10-13 – ": : : this would imply that biota add CO_2 to the atmosphere in the oligotrophic regions..". No, it doesn't imply that. It would make such an exchange more avorable, but CO_2 is added to the atmosphere only if the delta pCO_2 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 pCO_2 data in the papers that address the metabolic issue.

Answer: Not changing our argument we will change the text in the revision to: "...this

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would imply that biota add CO_2 to surface water, contributing to a source of CO_2 to the atmosphere." No CO_2 data are needed to infer such a straightforward conclusion about the effect of the production/respiration balance on the CO_2 balance in surface water. Also it does not matter whether a net uptake of CO_2 is observed locally, i.e. due to the balance of biotic and physico-chemical processes (warming/cooling etc.). For example, in a general situation characterized by net CO_2 uptake any net heterotrophy would relax the air-sea pCO_2 difference and reduce the uptake rate respectively, ending up with more CO_2 in the atmosphere and less CO_2 in the ocean. Since, as the reviewer remarked, we have no CO_2 data, we shall not dwell on this point any more and leave it at the hint given in the Introduction. We shall state more clearly that heterotrophy, while increasing pCO_2 and decreasing the air-to-sea CO_2 partial pressure difference and the associated air-to-sea flux, this does not necessarily reverse the sign of the exchange flux.

Reviewer: 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.

Answer: This will be made clearer in the revision.

Reviewer: 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.

Answer: True. There will be no more talk about burning ships in the revised version. In the Materials and Methods section it will be made clear what samples were collected when.

Reviewer: 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?

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Answer: Since we have no water age, we can only present gradients. Other workers have gradients and water age to construct rates of reaction from both. Still we can compare our gradients with theirs. We will change the text to make this clearer.

Reviewer: 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.

Answer: We shall improve the Materials and Methods section accordingly.

Reviewer: 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.

Answer: We do not use data points from the surface mixed layer, which we will make clear in the manuscript revision. All data used for the isopycnal analysis are from depths greater than ˜90m. The attached profiles (Fig. R1 of this text) reveal that the actual surface mixed layer depth is less than ˜75m (based on the sharp gradients of fluorescence one might argue even less than 50m) even at the two stations with slightly negative AOU in the uppermost isopycnal.

Bubble injection is only one of a number of processes producing AOU (or O2 supersaturation) and supersaturation is no proof of water contact with the surface. Photosynthesis, solar heating and mixing (due to the nonlinear relationship between temperature and oxygen solubility) below the surface mixed layer depth are also possible causes. More specifically: a model study by Dietze and Oschlies 2005 showed that supersaturation of several percent, as observed in the vicinity of station BATS, may well be driven by abiotic processes other than bubble injection below the surface mixed layer.

Reviewer: 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.

Answer: Note that Jenkins 1987 does also include negative AOU (his Fig. 18 A). We did not reject the samples with (slightly) positive AOU also to stay consistent with the Jenkins 1987 estimate. Note, however, that a possible error introduced this way would be small (less than ˜10% for the uppermost isopycnal and less than 3% of the vertical integral according to Dietze and Oschlies 2005). Furthermore, even excluding the respective data points would change the slope of the regression lines only slightly but not the ratios (e.g. AOU:NO_3).

The identification of the mismatch of OUR and nitrate supply requires an age tracer. It is correct that in order to estimate each of OUR and nitrate supply we would need an age tracer. However, in a ratio age cancels out. The ratio of OUR and nitrate production is the same as the ratio of delta AOU and delta nitrate, as both oxygen and nitrate should follow the same water mass ageing at the same rate for both tracers. When comparing our work with that of Jenkins (1982, 1987) we compare gradients, not rates.

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Reviewer: 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.

Answer: We did not include surface waters as pointed out above. The statement of the reviewer that the gradient is essentially based on the data at these points is not true.

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

Answer: We regret this omission. The figure legend will be completed in the revised manuscript.

Anonymous Referee #2

Reviewer: These authors are presenting results from a cruise in the Eastern North Atlantic Ocean, conducted in an area reported by others to be one of imbalances in production and consumption of organic matter, and of unaccounted-for nutrient fluxes. It appears to be an area of carbon overconsumption, or, where the oceans seems to be operating heterotrophically – producing more carbon than can be explained by nutrient fluxes, and therefore, releasing more carbon dioxide to the atmosphere that there is (apparently) being taken up from the atmosphere. I am not familiar with this body of literature or this part of the world ocean, and thus my comments below should be viewed accordingly.

Their approach, which is at the heart of their paper, of course, seems to me to be a good one. Instead of laboratory incubations to measure rates of organic matter production and respiration, etc., they are taking a large-scale field approach. They purport to follow changes in targeted variables (AOU, TOC, and dissolved organic and inorganic nutrients, etc.) along what they presume to be a "flow continuum" in the ocean (a current system) and to look for changes downstream that have occurred over a time-space scale of months and hundreds of miles. They present their results as plots of each variable for waters of constant density anomaly (sigma-t), along a transect at 30°W longitude, from about 31°N to about 18°N, a distance of roughly 800 nmi. And they perform linear regressions of those variables with distance. So far so good, except that the slopes of their regressions seem to be quite small and are given without confidence limits, and thus they may not be significant.

But, what jumped out for me was that their most important assumption may not have been met – that they may not, in fact, have been following a streamline in a current system. Their plots of sigma-t show that this was probably not the case, in that the vertical density structure is very different between the northern and southern ends of their transect. The southern end is more stratified, with much higher nutrient concentrations below ca. 200m, whereas the northern end is not as well stratified, and deep nutrient levels are half those in the south. But even more striking is the fanning outward of sigma-t isopycnals toward the north, which would signal a change in the character and direction of the geostrophic current field there. That is, wouldn't there be a current at the northern end of their transect that flows out of the page (e.g., top panel in Figs 2-6)? That is, hasn't their transect in the north begun to bisect the Azores Current (e.g., their Fig.1) that flows perpendicular to the transect?

Answer: We are aware that thermocline waters in the subtropical North Atlantic are a mixture of several water masses, not all of them ventilated in the North Atlantic. In Dietze et al. 2004 (cited in the original manuscript) we point out that the concentration of South Atlantic Central water is abruptly increasing towards the South. For this reason we do not include any data south of 23N in our analysis (not to 18°N as the reviewer writes). That is, the three southernmost stations are not included in the regressins of Figs.2-7. We thought that this was pointed out clearly enough in the original manuscript, but will explain this in more detail in the revised version. We shall also give an additional hint in the respective figure legends to avoid misconceptions.

As far as the Azores Current is concerned, a snapshot from the MERCATOR project (http://www.mercator-ocean.fr/html/mod_actu/public/welcome_en.php3) (which among other data assimilates sea surface height as viewed from space) does not indicate that the northern end of our section is in the core of the Azores current. It shows, however, that the reviewer is right in pointing out that there are currents flowing perpendicular to the transect. We know, however, that on scales considerably bigger than the eddy scales (which applies here, since the stretch of transect considered in our study extends a couple of hundred kilometers) we can reasonably assume that the age, i.e. the time elapsed since a water parcel left the surface mixed layer, is higher in the South than in the North on given isopycnals. This point is confirmedby Jenkins 1987. Furthermore, we know that the outcrop of the two uppermost isopycnals considered here are still within the oligotrophic gyre and should, irrespective of the exact location, be subducted with similar preformed values (i.e. nutrient concentrations and apparent oxygen utilization close to zero). Hence, even though our transect is not perfectly aligned in the direction of water movement, the following assumption, concerning the two uppermost isopycnals considered here, is valid: Using an adequate spatial average, the northern stations sampled younger waters than the southern stations. As the preformed values of all waters found in the South (but still north of 23N) should be very similar since they all originate in the oligotrophic subtropical North Atlantic which is rather homogenous in terms of surface nutrients and AOU) we conclude that differences between the North and South (I.e. 31 vs 23°N) are in fact imprints of biogeochemical processes in the region.

To meet any potential doubts of future readers concerning these points we will include a paragraph on this matter in the revised version of the manuscript.

Reviewer: The authors have not attempted to reconcile this possible flaw in their thinking; there is no discussion or presentation of the hydrography in this region. I would suggest they recruit the assistance of a physical oceanographer and reconsider this aspect. From what I could follow re: their biogeochemical arguments, they know what they're talking about, and they may in fact have a case to present. But I believe it all hinges on their supporting the physical basis of their most important underlying assumption – that they are following a bolus of water in a current system.

Answer: We will include a detailed discussion of the hydrography and implications for our study in the revised version. We think that a more concise treatment of hydrography with explicit reference to the necessary preconditions of our approach will help to convince the reader that our approach is valid.

References:

Jenkins, W.J.: 3H and 3He in the Beta Triangle: Observations of gyre ventilation and oxygen utilization rates, J. Phys. Oceanogr., 27, 763-783, 1987.

Dietze et al. Internal-wave-induced and double-diffusive nutrient fluxes to the nutrient-consuming surface layer in the oligotrophic subtropical North Atlantic. Ocean Dynam 54, 1-7, 2004. http://www.springerlink.com/content/h4dvaw9nf9y27g6v/

Dietze H., and Oschlies, A.: Modeling abiotic production of apparent oxygen utilisation in the oligotrophic subtropical North Atlantic. Ocean Dynam, 55, 28-33, 2005. http://www.springerlink.com/content/lfn6vm7f966dv6rn/

Fig. R1 (of rebuttal): CTD-data of two selected stations from the 2002 cruise. Station coordinates are indicated in the plot. Temperature (red), salinity (black), uncalibrated chl-a fluorescence (green). Oxygen-sensor data shown are corrupted and to be ignored. All oxygen data used in the manuscript are from Winkler titrations.

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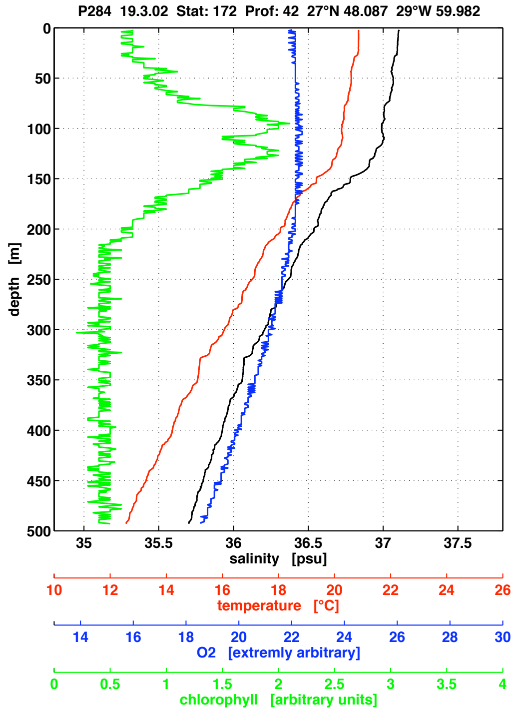

Fig. 1. R1a (of rebuttal), See text for caption.

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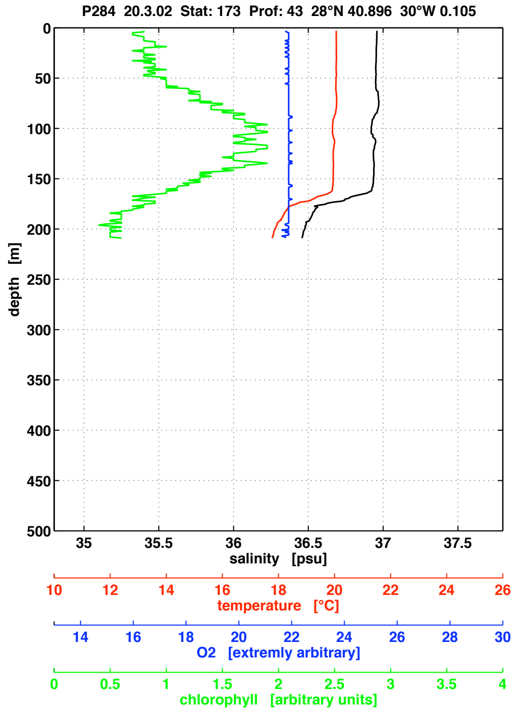

Fig. 2. R1b (of rebuttal), See text for caption.

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