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Interactive Comment

Interactive comment on "Apparent oxygen utilization rates calculated from tritium and helium-3 profiles at the Bermuda Atlantic Time-series Study site" *by* R. H. R. Stanley et al.

Anonymous Referee #2

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General comments

The manuscript by Stanley et al. presents an estimate of the apparent oxygen utilization rate (AOUR) in the North Atlantic subtropical gyre, based on recent measurements of oxygen concentration and a careful estimate of water mass ages from 3H/3He data.

AOUR forms the basis of a widely used method of estimating export production from the surface layers to the ocean interior – providing a measure of the strength of the biological pump. The AOUR estimate by Stanley et al. corroborates existing estimates based on similar methodological approaches, but is larger than export estimates based on in situ trap and radionuclide data. This discrepancy is well known in the oceanographic community (see for example Burd et al., 2010), and reflects both the difficulty of extrapolating measurements localized in time and space to large spatial and temporal scales, and our incomplete understanding of the export processes that constitute the biological pump. Therefore, large-scale geochemical estimates such as the one presented by Stanley et al. represent a useful benchmark against which export estimates from direct methods can be compared.

Ocean oxygen varies in concert with ocean circulation and climate and has been suggested to decrease as a consequence of climate change (Keeling et al., 2010, Deutsch et al., 2011). AOUR measurements from different decades, of the type presented by Stanley et al., can be helpful in separating solubility, ventilation and biological effects on low-frequency oxygen variability. Indeed, the data by Stanley et al. show an increase in AOUR from the 1970s and 1980s to the 2000s. By comparing water mass age estimates from 3H/3He measurements from the 1970s and 1980s to the new data, Stanley et al. show that water mass ventilation did not change significantly, and that instead AOU increased. The conclusion by Stanley et al. is that, quite surprisingly, measurement artefacts are responsible for increased AOU compared to the 1970s. Given that the observational evidence for ocean deoxygenation relies on oxygen time series, the suggestion of methodological artefacts is very provocative – although still speculative – and calls for further investigations and great care in interpreting historical oxygen data.

The manuscript by Stanley al. addresses current problems in ocean biogeochemistry, with a robust combination of data collection and analysis. I support the publication of the manuscript in Biogeoscience Discussions. That said, I have a number of relatively minor comments that the author should be able to address in a revised version of the manuscript.

Specific comments

Section 2

1. Overall the method used to estimate water mass ages from 3He/3H by using a tran-

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sient time distribution TTD approach is sound and well described. Tracer ages derived from different tracers present differences that depend on the tracer boundary conditions, sources and sink distribution, and the characteristics of the flow (e.g. Waugh et al., 2003). However, the first moment ('mean age') of the transit time distribution, is an intrinsic characteristic of the flow, and should be independent of the tracer considered. Stanley et al., adopt the 'mean age' to calculate AOUR from AOU (equation 7). Whereas I have no major concerns about this method, I think it would be worth clarifying the assumptions that allow the use of the 'mean age' in equation 7. Ideally – if it was available – one could use an 'AOU age' – depending on both circulation and oxygen sources and sinks – that is the tracer age that would be inferred from AOU measurements if one were to exactly know the sources and sinks of oxygen in the ocean interior. Put it another way – what is the correct age to be used in equation (7), and to what degree is the 'mean age' a good approximation to it? Similarly to the transient time, the AOU of a water parcel should be interpreted in a probabilistic way, and not necessarily the density distributions of transient times and AOU would coincide. These are points beyond the scope of the paper but it would be useful to see them discussed with more detail.

2. Page 9982, equation (2): I am surprised that in the updated source function for tritium no confidence bounds are provided for the regression coefficients. This should be straightforward to include. Additionally, they could be included in Figure 1, which perhaps would benefit from being extended to the 1950s, to include Dreisigaker and Roether (1978) source for BATS. On a note, the empirical source function is not strictly speaking an exponential (page 9982, line 21), but the sum of an exponential and a linear trend.

3. Page 9983, line 19 and following. The TTD is by definition the Green's function of the advection-diffusion operator that propagates surface boundary conditions into the interior. The specific form for the Green's function chosen by Stanley et al. after Waugh et al. (2003) is an inverse Gaussian function. The assumption of inverse Gaussian

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TTDs for the upper ocean in the subtropical gyre seems reasonable, given the patterns of water-mass circulation that characterize the region. Nonetheless, the section would benefit from a discussion of why this specific TTD has been chosen.

4. Page 9984, line 8-9: this sentence is incomplete. What is the assumption adopted here?

5. Page 9984, line 22. It should be clarified why the 3He data give a more precise and robust determination of Gamma_best than 3H. I find it confusing since from Fig. 2 and section 3.2 it seems that in the upper water column the relative errors on 3H are smaller than the errors on 3He, and the equations used for the convolution should be similar. Also, why is it not possible (or worth) using 3He and 3H simultaneously to estimate Gamma_best?

Section 3

6. Page 9987, line 5. The reference to mean age (tau) variations in figure 5b is misleading, since figure 5b shows AOUR and not mean age values.

7. Page 9987, line 13-15. The sentences 'The box model approach has an implicit exponential shape to the water mass probability distribution' and 'the TTD model ... is mixing waters with a larger age spread and has a non-zero centroid' are confusing. Could these be clarified or rephrased?

Section 4

8. Page 9991, line 18 and following. It should be clarified that the transport matrix method referenced in Kathiwala (2007) is based on model simulations, and the water mass contributions determined with this method depend on model-simulated circulations (albeit the transport matrix method has been applied to data-assimilating models). The total transport matrix method detailed in Gebbie and Huybers (2010) is perhaps a more relevant reference that is completely based on tracer observations. See for example figure 9 in Gebbie and Huybers (2010).

9. Section 4.3. The central point of this section is the suggestion that methodological artifacts are responsible for the increase in AOUR from the 1970s-1980s to the 2003-2006 periods. Whereas I find the combination of figure 7.b and 8.b suggestive of the possibility of biases in earlier O2 measurements, I do not think the evidence is strong enough to conclude that "... the apparent differences in AOUR between 2003 and 2006 and the 1980s ... is likely due to methodological artefacts". I do agree that the result calls for both caution in the interpretation of O2 time series and further analyses of earlier O2 measurements. In particular inspection of figure 8 alone does not fully convince me that the O2 difference is completely a methodological artifact. Figure 8 and the discussion in section 4.3 do not allow to assess whether there is a bias in the late 1980s measurements at Station S (blue circles), or a bias at Station 50 on the Endeavour 129-1 leg. Measurements at Station S show a decreasing O2 trend between mid 1980s and mid 1990s when they start to overlap with BATS measurements. Yet no indication is provided as to what methodological artefact could be responsible for this decrease, and why BATS measurements should be trusted more than Station S measurements. Without a detailed intercomparison of O2 measurements from the different programs – including considerations on the analytical techniques, formal statistical time series analysis on O2 (for example change-point detection), and analysis of the variability of the regional hydrography – the hypothesis of methodological biases in early Station S O2 is speculative. See also comment 11.

10. Section 4.4. I wonder if it would be possible to include the information provided by Delta, the width of the TTD distribution, in the estimate of the AOUR uncertainty. In a sense, the knowledge of the TTD should allow the estimate of a density distribution for AOUR (ignoring the issue of the density distribution of a water parcel AOU). For example, a TTD with a wider Delta should imply a larger range of uncertainties for the AOUR.

Conclusions

11. Page 9999, lines 3-5. As noted in point 8., I do not feel that at this stage the

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evidence provided is sufficient to conclude that “this increase is due to an increase in AOU and is more likely associated with methodological artifacts in the oxygen data from the 1980s”. However, this is an important possibility that the data do suggest and that should be further investigated. I suggest that the sentence be rephrased.

Figures

12. Fig. 3, Fig. 4, Fig. 7. The figures would be much easier to read with a different aspect ratio – that is a wider x-axis, as most of the information in the upper water column is squeezed to values close to zero. Could the authors re-plot them exanding the x-axis?

Technical comments

1. Page 9979, line 24: I don’t think Khatiwala et al., (2009) is the appropriate reference. Waugh et al. (2003) is sufficient.

2. Page 9980, line 21: delete additional “the”.

3. Page 9983, line 20: Green’s and not Greens.

4. Page 9984, line 4: the Peclet number should be analogous to (Gammaˆ2)/(Deltaˆ2) and not to (Gamma/Delta) (e.g. Waugh et al., 2003).

5. Page 9988, line 24: sensitivity instead of sensitvty.

6. Page 9988, line 27. Remove the comma after AOUR-derived.

7. Page 9991, line 2-3. Should it be “differences between AOUR and OUR” instead of “Differences between AOUR and AOU”?

8. Page 9991, line 16. Change “as well as sources from the southern ocean” to “as well as in the Southern Ocean”.

9. Page 9994, line 23-24. 1977-1987 instead of 1977-1877.

10. Page 9996, line 4. Add ‘the’ before ‘source function’.

References

Gebbie and Huybers, 2010. Total Matrix Intercomparison: A Method for Determining the Geometry of Water-Mass Pathways. Journal of Physical Oceanography 40.

Interactive comment on Biogeosciences Discuss., 8, 9977, 2011.