We thank the referee for his comments. Below is our response. Blue text signifies a direct quote from the referee's comments.

First, the conclusion that the AOUR could have increased over the last several decades is flawed as the authors themselves point out the problem with the old oxygen data. There is no substantial reason to speculate that respiration has increased by such a large amount. One should not draw a conclusion based on highly uncertain data.

We agree with the referee that the most likely cause of the observed change in AOUR is methodological artifacts associated with the older oxygen data rather than any real changes in the ocean. Indeed, even in the original version of the manuscript in the section discussing oxygen change with time, we stated this *"Examination of the deep oxygen record (2100 to 2700 m) from Station S and BATS over the last thirty years supports the conclusion that the difference in AOU is likely due to methodological artefacts (Fig. 8)."*

In the revised manuscript, we have modified the conclusion to firmly state that the AOUR increase is most likely due to problems with old oxygen data *"We compared AOUR presented in this study to AOUR calculated based on earlier tritium and ^{3}He data and found a large increase in AOUR over the past thirty years. Although a simple interpretation of the data would suggest a major change in production or remineralization, such a change is not likely. Instead this increase in AOUR is most likely due to methodological artifacts in the oxygen data from the 1980s. This suggests that historical oxygen data should be used with caution when making interpretations about ocean deoxygenation."*

We believe it is very important to include the comparison of AOUR found in this study to AOUR calculated from previous decades in order show the problems with making such comparisons based on potentially flawed data. Many studies are being published using old oxygen data and this work should serve as a cautionary tale, showing that such old data must be examined critically.

Secondly, I feel that the introductory paragraph needs a revision as it trivializes observed oxygen changes as a simple manifestation of global ocean de-oxygenation. Authors make a reference to [Deutsch et al., 2011] on page 9978 line 16 in the context of de-oxygenation. However, what Deutsch et al discusses in that paper is more complex than just a monotonic trend. Rather, the size of tropical Pacific Oxygen Minimum Zone is correlated with the Pacific Decadal Oscillation through the reinforcing changes of upwelling and oxygen utilization in the thermocline.

We have modified the introductory paragraph to reflect the issue commented on by the reviewer.

Third, I really like the discussion in page 9980 line 3-6. It is very important that the role of horizontal ventilation at the water parcel can pass through different biological regimes. Therefore, the data presented in this paper is not relevant to the vertical profile of sinking particles. I don't see a point in section 4.1.

We agree that the data does not have one-to-one correspondence to the vertical profile of sinking particles. Indeed, we make that point ourselves. But AOUR as a function of depth is still reflective of vertical processes of particle sinking and remineralization – it is just that these processes are occurring over a wider spatial scale than would be in the case of a sediment trap.

Additionally, AOUR has been linked in the past to vertical changes and thus it is important we clearly state to what extent that is useful and how much a bias a strict vertical interpretation would have. We have substantially restructured section 4.1 in response to the referee's concerns. We now start with a description of the extent to which AOUR can be construed as a vertical process, noting the biases that a strict vertical interpretation would cause. We end with a brief description of the Martin et al curve fit. We agree with the reviewer that applying a Martin et al curve is not correct and we clearly state this in the paper. However, we fear if we do not do it, people who are not used to the nuances of AOUR, will try to fit a Martin curve to the data and will not understand why that isn't a good idea. Thus it is better for us to fit the curve and explain the shortcomings of the Martin et al model for this dataset.

Fourth, "relatively small" is not very convincing on page 9983 Line 4. It would be reassuring to see that spatial variation of surface TU is indeed small (or not?) relative to the signals of interest. Some of the discussion in section 4 should be moved up here.

We have changed "relatively small" to 0.01 to 0.2 TU for 10 degrees of latitude. We calculated this by using tritium data from surface water (<25 m) during the 2003 Repeat Hydrography A20 and A22 cruises to obtain a range of 0.01 to 0.15. Also, in the Doney and Jenkins, 1988 paper, Figure 2a shows about a 0.2 TU difference for 10 degrees of latitude. We reference that paper. We refer the reader to the part of section 4 that concerns the uncertainty due to variations in the source function. We did not move that discussion here in order to keep the manuscript organized so that all the uncertainty analysis is in one place (Section 4) and all the description of calculations is in another place (Section 2). Since we make a reference to Section 4, a reader who wants to immediately know the effect of this variation in source function can jump ahead to the appropriate section.

(Technical issues)

Page 9984, line 4, gamma/delta is analogous to the square root of the Peclet number (Pe). For the one-dimensional advection-diffusion equation with constant coefficients, the ratio is exactly equal to the square root of Pe.

We updated the paper to reflect this.

Page 9984, line 7 is an incomplete sentence. Please be explicit about the assumption between gamma and delta.

The assumption is that gamma/delta = 1.

Page 9984 line 21. Please clarify that the best fit is still subject to a specific choice of gamma/delta relation. This could be a part of uncertainty analysis.

Where we first introduce the concept of gamma/delta, we explain that the results are sensitive to the choice and that an uncertainty analysis will follow where we examine different gamma/delta relations (lines 9-12 in the original version of the manuscript). Thus the reviewer's request is already present in the manuscript. Nonetheless, we added a few lines where the reviewer requested them: "τ is sensitive to the choice of Γ/Δ and thus the uncertainty added by the choice of Γ/Δ is included in the uncertainty analysis (see section 4.4). "

Page 9988 line 4, please remove "a".
We did.

Page 9990 line 25. It does not make sense for the isopycnal trajectory to "decrease". Do authors mean "deepens"(i.e. move downward) instead of "decreases"?
Yes. We changed the wording to say depth of the isopycnal increases linearly with time.