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

The authors correctly explain the fact that oxygen utilization observed at a given depth at the BATS site should not be seen as being caused by vertical processes(such as carbon export and remineralization) at the BATS site alone, but has to be interpreted as the integrated result of remineralization and oxygen utilization along the entire path of the water mass from the outcrop regions further north and northeast to the observation site. Thus, the estimated AOUR rates and export fluxes represent regional estimates of productivity and remineralization (as stated in the abstract), rather than local fluxes or rates at the BATS site.
Determining the exact areas of "influence" is difficult and would require knowledge of the specific water spreading pathways in the region. Maps of outcrop regions (Fig. 6) suggest path lengths of several thousand kilometers for the thermocline waters at BATS. Biological productivity in the outcrop regions (north and northeast of BATS) tends to be much higher compared to productivity at BATS, and the observed AOU values at BATS to a large extent appear to reflect productivity and remineralization further north and northeast.
The following parts of the paper are inconsistent with above interpretation:
• Sentence p 9998/l 19-21in the Conclusions implies that a local flux has been estimated.

We have modified the sentence in question to remove the "at BATS" and also have moved it to after the sentence saying that the export is reflective of large spatial and temporal scales. Thus it should be clear that we are not discussing a local flux.

• Table 1 and section 4.2 imply that present export fluxes can be compared with independent estimates of carbon export based on sediment trap and 234Th data. However, due to the relatively small statistical funnel of shallow sediment traps and the short half-life of 234Th, the latter estimates represent "truly" local fluxes, which should not directly be compared with AOUR derived estimates. In fact, the discrepancy between both methods could be explained by the contribution of high-productivity areas to the north and northeast of BATS on AOU.

We have expanded the discussion of the table 1 and section 4.2 to clearly explain one needs to remember the difference in spatial scales when comparing estimates of production, adding in several sentences to that effect. In particular, the first "confounding issue" we now mention is the spatial scale and the higher productivity to the north and northeast. We believe the table is still pertinent since scientists are interested in export production at different scales and thus we are still including the table.

• Because of the regional effects discussed above one would not expect a Martin curve for AOUR or export flux, but rather a projection of the spatial (meridional) productivity gradients onto the vertical. So, the discussion on deviations from Martin-like vertical profiles in section 4.1 appears artificial and should be deleted.

We have substantially restructured Section 4.1 in response to the referee's concerns. We have moved the discussion of the Martin et al curve to the very end of section 4.1. As we stated in the paper, the AOUR profile incorporates information on the vertical profile of respiration as the depth of the density surfaces varies along path and thus it is relevant to talk about other vertical models of respiration. As we noted above in response to reviewer #1, we agree that the Martin et al model is not a good one. However, we believe that if we do not mention it, someone else might try to fit the model. Thus it is better for us to do it and clearly state the shortcomings of the model for this dataset.

• The plots of tritium and 3He data from the upper 500 m at the BATS station (Fig. 9) are used to confine the □/ parameter of the TTD. This would be valid if the predominant transport (advection and mixing) was vertical (diapycnal). Under the paradigm of predominant transport along isopycnals(see above) plots of tritium and 3He on isopycnals would be required. It is unclear what conclusion can be drawn from Fig. 9.

We have modified the text of the paper and the figure caption to be clearer since the reviewer misunderstood the figure and discussion. Figure 9 and the conclusions drawn from it do not require that the mixing is diapycnal as opposed to isopycnal. Rather, the figure is used to show what the ensemble of Γ predicted by the TTD approach for a fixed Γ/Δ looks like in ^{3}He/T space. In addition, it shows what the actual shallow data at BATS is in ^{3}He/T space. Since the actual data is consistent with a range of Γ values, we can compare the two to get a reasonable idea of what a suitable range of Γ/Δ is. We cannot use this approach to exactly pinpoint Γ/Δ but it does give us some estimates of reasonable range of Γ/Δ so we can perform the uncertainty study. We find a range of Γ/Δ (0.8 to 1.1) that agrees well with the range used by Waugh et al (2004) in the North Atlantic (0.8 to 1.33).

· It is unclear whether specific TTD forms derived for 1D advective/diffusive are applicable to the thermocline circulation of the North Atlantic and whether the gamma/delta range in reality is much larger than the one assumed.

Waugh et al (2004) and Hall et al (2004) show that inverse Gaussian TTDs with $\Gamma/\Delta=1$ could reproduce tracer distributions in the Atlantic subpolar gyre and the Indian Ocean. Additionally Jenkins (1988) showed that the Peclet number is approximately 1 for the North Atlantic subtropical gyre. Since Γ/Δ is equal to the square root of the Peclet number, then Γ/Δ should be approximately 1 as well. In Waugh et al (2006), the authors use a range of Γ/Δ of 0.8 to 1.33 (they refer to Δ/Γ range of 0.75 to 1.25) as a reasonable range. In the initial draft of this paper we used 0.8 to 1.1 as well as showing the results for 2.0. In the revised draft, we now use a Γ/Δ of 1.33 as our upper reasonable limit and show that it yields a 6% error (as opposed to the 5% error we estimated before). We have updated Table 2 to include the case of Γ/Δ of 1.33 and cite the Waugh et al 2004 and 2006 papers.