

Reply to comments by Referee #2

The comparison with the MLD definition from Talley (1999) could be left out. One wouldn't expect good agreement when using a technique that was proposed for winter MLD.

Response: Following this suggestion, we will remove Figure 6, which showed a comparison of $z_{\text{mix}}(\text{O}_2)$ based on our 0.5 % difference criterion to the 95 % saturation criterion of Talley (1999). We will also remove the comparison against this criterion in previous Figure 4. The later will be re-numbered as Figure 5 in the revised manuscript. However, we decided to leave the verbal description of the comparison in the body text because as far as we know Talley's work is the only published use of O_2 to define mixed layer depth.

When comparing the different criteria at the in-situ profiles I would include an additional calculation: the Lorbacher (2006) criterion is not limited to temperature and density, but can also applied to the oxygen profile.

Response: We have tried what the reviewer suggested and found the result to be comparable to the results based on the simpler relative difference criterion. However, the detection of the uppermost curvature could lead to the false detection of seasonal mixed layer depths. By using the Lorbacher criterion in oxygen profiles, the mixed layer depth is underestimated (by as much as 17 ± 15 m) and this is probably due to the presence of Winter Water in the upper water column of some CTD stations. A corresponding note will be included in the revised manuscript. The corresponding results of this analysis will be depicted in Figure 5 of the revised manuscript.

The "methods" part needs major improvement. The data processing of the oxygen data from the SBE43 sensor should be described in detail. On page 1511, lines 7-10, the authors estimate the sensor precision from the readings in 2 dbar intervals. This precision depends strongly on the response time of the sensor and the speed of the CTD during heave or veer. Another point that is not discussed is how the authors deal with the hysteresis. The oxygen sensor shows a much bigger hysteresis effect than the temperature and salinity sensors, which again depends strongly on the CTD speed. This has to be taken into account and should be described in the manuscript.

Response: We will clarify this section as follows:

"The O_2 sensors responded less rapidly than the other CTD sensors. Sensor lags of 8 s and 9 s were established for the two sensors by finding the lag time that minimised the mean and root mean square differences between the downcast and upcast. Plots of downcast and upcast alignment were used as a visual check for the quality of the correction. O_2 profiles in deep CTD casts > 1000 m were reported to be affected by pressure hysteresis (Sea-Bird Electronics, 2010). However, little hysteresis was observed in our profiles, since only 38 of them reached depths greater than 1000 m. A small correction had to be applied to the pressure sensor, which amounted to the following values: CTDs 000-021: -1.21 dbar; CTDs 022-085: -0.71 dbar; CTDs 086-164: -1.05 dbar and CTDs 165-253: -0.80 dbar."

Specific comments:

1507- 5: Looks like there is a space too much between "immediate. Likewise"

1507-14: one "ratio" too much

1507-21: no dot between " : : ocean (Rintoul : :"

1510-13: I would refer to Fig. 1 here and not in line 20

Response: We will change this as suggested.

1511-7-10: This number depends strongly on the time constant of the sensor and the velocity of the CTD during up- or downcast (see above)

Response: Since $c(\text{O}_2)$ is stable in the mixed layer, we expect this value not to depend on sensor response time and CTD velocity.

1512-2: the "of" has to be deleted

1512-21-24: I'm confused. Please compare with Table 1, it looks mixed.

1513-9: The “(“ before Antonov has to be deleted

Response: This will be corrected as suggested. There was a typo in the first line of Table 1.

1513-21/22: Why using the subjective MLD? It's not comparable to the other criteria that are objectively defined.

Response: We cannot think of an alternative way to establish potentially suitable z_{mix} criteria other than the human eye and brain. Presumably, the same route was chosen to define previous temperature- or density-based criteria. Of course, the detailed evaluation of potentially suitable criteria and their comparison to other methods then has to be (and indeed was) done using numerical algorithms. Whether the subjective or objective $z_{\text{mix}}(\text{O}_2)$ values are used for comparison purposes is a matter of taste, but following similar queries from both reviewers, we are now using the objectively defined $z_{\text{mix}}(\text{O}_2)$ values for comparison. Previous mean values will be updated by the comparison to objective $z_{\text{mix}}(\text{O}_2)$ in the body text accordingly, and in Table 2.

1515-13-16: This sentence makes no sense, please rewrite.

Response: This will be rewritten as: "Thus, $z_{\text{mix}}(0.125 \text{ kg m}^{-3})$ was compared to z_{mix} from the ML97 climatology. The same procedure was applied for $z_{\text{mix}}(0.03 \text{ kg m}^{-3})$ and $z_{\text{mix}}(\Delta\sigma_\theta(0.8 \text{ }^\circ\text{C}))$ in the BM04 and K03 climatologies, respectively. z_{mix} extracted from the climatologies corresponded to the same month and geographical location of our CTDs."

1516-16: Due to the number of different criteria it's not clear if " $z_{\text{mix}}(0.03 \text{ kg m}^{-3})$ " the BM04 climatology or the difference criterion for a profile.

Response: This depends on the context and will be made clear in the revised manuscript, e.g. in Table 2.

1517-23: The abbreviation "ML" occurs the first time here.

Response: We do not use this abbreviation elsewhere and will be replaced by writing explicitly "mixed layer".

1518-22-25: The statement sound a bit too hard. Looking at Fig. 4, the Lorbacher criterion is not much worse than $\Delta\sigma=0.03 \text{ kg m}^{-3}$ criterion.

Response: We agree, but the Lorbacher criterion has not found wide acceptance so far, perhaps because of its novelty and slightly more involved calculation method.

1519-18: "of" can be deleted; a short statement why the diurnal variability can be neglected might be good.

1520-24: should be "interest"?

Table 1: Please check with text. Numbers do not agree.

Response: This will all be corrected in the revised manuscript.

Fig. 1: A colourbar for the shading instead of text would improve the figure.

Fig.2: The explanation (dashed line: : :) is not needed as there is a legend. Is the $z_{\text{mix}}(\text{O}_2)$ the objective or the subjective MLD?

Fig. 3: The calculation of $z_{\text{mix}}(\text{O}_2)$ is not a numerical algorithm.

Fig. 5: The figure is not required to understand the text. But if it should stay: one panel seems to be enough; please explain the marker in the figure caption OR use a legend.

Fig. 6: not needed.

Fig. 7: please explain the marker in the figure caption OR use a legend; caption of the y-axis must be " Δz_{mix} "

Response: We accept the reviewer's comments for improvement on the figures.

Figure 5 will be removed. In replacement, we have created a more explicit figure (Figure 4 in the revised manuscript) where is depicted the difference between $z_{\text{mix}}(\text{O}_2)$ minus the z_{mix} by

the tested potential density criteria (0.03 and 0.125 kg m^{-3}). In addition, the difference between the objective minus subjective result of $z_{\text{mix}}(\text{O}_2)$ previously presented in Figure 3 of the discussion paper.

Figure 6 will be deleted and a new figure will be added (as new Figure 3 in the revised manuscript) in order to clarify the different range of O_2 difference criteria evaluated to come to the agreement of the selected criteria. These figures are enclosed to this author comments.