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Interactive comment on "Oxygen and nutrient trends in the Tropical Oceans" *by* Lothar Stramma and Sunke Schmidtko

Anonymous Referee #2

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The manuscript describes changes in dissolved oxygen and nutrients which might have various and important impacts on the ocean ecosystem. As the authors note, the manuscript confirms conclusions done in the previous work by Stramma et al (2008), which describes trends in the same selected geographical areas.

1) My main concerns regarding the significance of the presented results relates to the poor observational basis. In the concluding remarks the authors themselves acknowledge this fact and point to the necessity for further verification in the future.

The visual inspection of the calculated trends shows high variability of the yearly parameter concentration values imposed on a much weaker climatological signal. Some of the trends are not significant at 95 percent level. The others, even formally significant, leave the impression that the removal of just few data points would lead to signif-

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Discussion paper

icantly different trend estimates. Here, increasing the number of investigated areas of similar size , or increasing the size of the investigated areas could help to confirm (or not to confirm!) the robustness of the presented results.

2) Using optimally averaged yearly values which in turn are used to estimate trends is justified, as the averaging procedure acts a a smoother. However, I would appreciate the elaboration about the possible errors related to the mapping scheme, and, because of the data paucity, even these averaged data might be linked to a relatively large errors.

3) The authors do provide measurement precisions for the modern measurements and offset estimates for the older data. The reference is done to the paper by Tahua et al., 2010.

Even for temperature which is easier to measure compared to other parameters, systematic instrumental errors pose a big problem in estimating the ocean heat content changes. Possible instrumental biases in oxygen and nutrient data is even a bigger issue, which was treated, for instance, during the WOCE time. Here, references to Johnson et al., 2001, and to Gouretski and Jancke, 2001 could be added to the reference list. From the manuscript text it is not clear, whether the original data were corrected for systematic biases, or not. More discussion of possible bias impact is needed.

4) Several areas show a certain shift in oxygen concentration for the data after 2000 (Fig.2a,b,d,f; Fig.3 a,e) Could these change in concentration be due to unexplored biases?

5) it is interesting to know, how sensitive the calculated trends are to the thickness of the layers (the fixed thickness of 250 and 400 meters are used for the presented results).

6) Can the yearly parameter values be seasonally biased considering the poorness of the data basis??

Minor comments:

1) I do not see much use in presenting mean parameter values for the investigated areas without providing the number of available original profiles and standard deviations (Table 3) 2) Please, indicate in the figure captions, that crosses denote the annual parameter values used to calculate trends

Line 13: change indicates to indicate

Lines 125-126: probably the word "interpolated“ is missing after the word "profiles“ (Line 126)

Line 267: change nutrient to nutrients

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