

## ***Interactive comment on “Evidence for greater oxygen decline rates in the coastal ocean than in the open ocean” by D. Gilbert et al.***

**D. Gilbert et al.**

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We thank referee 1 for his/her careful reading of the manuscript, and for making several useful suggestions that improved the paper. We also updated the manuscript by citing new papers that were published while the manuscript was under review (e.g. Cui and Senyu 2010, Keeling et al. 2010, Stramma et al. 2010).

RC = Referee COMMENT

RC. The extent to which oxygen declines may be a general phenomenon across coastal and oceanic systems is a pressing question. A number of recent synthesis papers have highlighted the variability of ocean oxygen content and the trend for accelerated appearance of hypoxia in coastal ecosystems as a consequence of eutrophication.

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Gilbert et al's analyses are certainly timely in these regards and represent a potentially valuable evaluation of global trends. The authors have focused their analyses on addressing 1) the potential for publication bias, 2) the generality of global oxygen trends, and 3) the potential for enhanced changes on oxygen trends in the coastal ocean due to eutrophication. While this work is extremely timely, one does have the general impression that this is a very cursory look at the dataset and that the concluding interpretations await a more statistical analyses. As I note in my specific comments below, the lack of statistical tests in support of the core conclusions are hard to overcome. In addition, there are potential biases in data selection that can be very easily be put to rest by a set of slightly more targeted analyses. This is an important start to the analysis of the extensive datasets that the authors have compiled. I look forward to the seeing the mature set of analyses that will make this a particularly noteworthy contribution.

RESPONSE. We recognize that our paper represents a first cursory look at oxygen trends in the global ocean. It is our best attempt at addressing within a finite timeframe one of the terms of reference of SCOR Working Group 128 on coastal hypoxia, namely, to synthesize the state of the science for spatio-temporal variability of hypoxia. We feel our paper represents a valuable first step in this direction.

RC. Section 1 –Solubility changes and export production can also play roles in lowering oxygen levels.

RESPONSE. It is true that solubility changes and export production can both play a role in lowering oxygen levels. In this first look at the data, we did not report on changes in solubility because many of the oxygen timeseries do not have accompanying temperature and salinity timeseries from which to compute solubilities. As for possible changes in global export production, the dearth of publicly accessible data prevents us from exploring this question in a meaningful way.

RC. Section 2.1 –The logic that a time series of 10yr duration allows one to remove the

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effects of interannual variability is clear, but the argument that the effects of decadal variability can also be accounted for by a 10yr duration window is tenuous. Why not directly acknowledge that interannual variability are minimized but that decadal variability remains a possibility that may be beyond the resolution of some portion of the time-series?

RESPONSE. Thank you for this suggestion. We accordingly modified the second sentence of section 2.1.

RC. I wonder if the possibility of introducing artificial jumps in the time series are indeed avoided altogether. Various aspects of the winkler method have evolved over time and for some historic low DO samples, there are known artifacts (see Broenkow and Cline 1969 for example) in the winkler approach (e.g. whether sodium azide was employed or not to control for nitrite interference, degassed reagents, collection by gravity flow versus syringe. . .etc.). These changes through time can be important. That's not to say that no effort should be undertaken to examine temporal patterns, only that the suite of caveats that can introduce biases and uncertainties are fully recognized and not assumed to have been avoided.

RESPONSE. We did not add the Broenkow and Cline (1969) reference to our paper. However, your point about changes/improvements to the Winkler method that have been made over the years is well taken. We chose instead to refer to the more recent paper by Wong and Li (2009) on iodate interference. We also mention two references (Carpenter 1965, Jones et al. 1992) pointing to oxygen titration techniques that post-dated the original Winkler method. Overall, these changes in titration techniques have mostly eased and speeded up the titrations, but the changes in oxygen concentrations induced by the changes in titration methods are negligible compared to other factors causing oxygen variations in the natural environment.

RC. Regarding the assumption of weakened variability and use of data from all months for stations >100m, this may be true in some systems but for coastal shelves where

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seasonal current shifts (e.g. eastern boundary current systems) are dominant hydrographic features, this would not be the most conservative assumption and would conceivably introduce a systematic bias in the ability to detect trends in deeper, off-shore stations. This matters of course as one of the goals in the paper is to compare nearshore versus offshore stations and boundary current systems are heavily represented in the data set. It seems that it would be simple to have consistent temporal criteria for data selection across the coastal, transitional and oceanic bins.

RESPONSE. Here a two-fold response is required. For section 2.1 that deals with published oxygen timeseries, we are totally dependent on the analysis methods used by the various authors. For section 2.2, where we have full control over the analyses, we have done seasonal analyses (e.g. summer data only) at 100 m and 150 m depth based on your suggestion. It turns out that while individual trend estimates from particular locations are indeed affected by using seasonal versus annual data, the overall conclusions of our paper remain unchanged. And an undesirable side-effect of using seasonally stratified data is that there are many more holes in the seasonal oxygen timeseries.

RC. Section 2.2 – I must confess that the wording on the temporal selection criteria confuses me. How is a standard reference period defined? Do you mean that there is a baseline period from which subsequent deviations are then calculated? The text reads more like a standard reference period is simply the period for which time-series data were used to derive a trend. I'm not sure what is standard and what is reference here.

RESPONSE. Considering the referee's confusion with our wording, we eliminated the expressions "standard reference period" and "reference period", replacing both of them with either "period" or "time period" .

RC. Also, if there was no year with data in one half of the time series, wouldn't that naturally shorten the time-series up to the year when data becomes available? How is

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1973 the middle year for the 1951 to 1975 time period? Some rewording should clarify these questions.

RESPONSE. Thanks for catching this mistake. We replaced 1973 with 1963 in the revised manuscript.

RC. I can certainly appreciate the caution in attempting to resolve trends for the extremely oxygen poor systems where interferences to the Winkler approach and the difficulty of detecting changes in an already small DO concentration. On the other hand, this can bias the outcomes of the meta-analyses depending on the distribution of excluded systems relative to offshore distance (e.g. such as central Black Sea, Cariaco Basin, the Humboldt current OMZ) and the influence of their temporal patterns on the global dataset. Since the emphasis of this paper is on the contrasts between coastal and oceanic stations, I find the exclusion of these low DO stations to be problematic.

RESPONSE. We have done some sensitivity tests, either including or excluding these very low DO timeseries stations. Our overall conclusions were unchanged.

RC. Also, I presume that all stations with H<sub>2</sub>S data were eliminated from the data set. If so, why the tenuous argument about using negative oxygen values and H<sub>2</sub>S conversion in the previous section?

RESPONSE. The reason why we preserved the argument about converting H<sub>2</sub>S to negative O<sub>2</sub> concentrations has to do with the published timeseries listed in Table 2, some of which have negative oxygen values (e.g. in the Baltic and Black Seas).

RC. Section 2.3 – At first glance, this seems to be a reasonable approach to adopt to group systems by their terrestrial vs oceanic influence. However, when one sees the heavy representation of the CalCOFI data set, it becomes apparent that this assumption may not be without bias. The strong trends that Steve Bograd described are included in the coastal band (table 4). The changes in those nearshore stations are not likely to be due to eutrophication (i.e. dominant ocean signal, low run-off). This is

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of course of concern because the differences between coastal and oceanic sites are important here for detecting the effects of eutrophication. I think that is a lot of room for moving beyond simple summary statistics in this paper. Evaluating the sensitivity of the statistical outcomes to inclusion or exclusion of systems seems like a natural step (i.e. the extent to which outcomes are stabilized...etc.).

RESPONSE. While it is true that the CalCOFI dataset is the most important publicly available dataset from the USA, it is not a dominant dataset in this global analysis of oxygen timeseries. We have far more oxygen timeseries stations around Japan and Europe for example. The 156 oxygen timeseries stations from the CALCOFI area (taken here as between 23°N and 39°N latitude, 110°W to 128°W longitude) represent 7.3% of the 2132 timeseries that were retained for this global ocean analysis. Nonetheless, we acknowledge that dividing timeseries stations in 0-30 km, 30-100 km and > 100 km distance bands from the coast is not universally relevant. We saw this approach as a first, admittedly rough attempt to see whether stations close to shore (more directly affected by river plumes) have overall oxygen trends that are different from those at offshore stations.

RC. Section 3 – Overall, the results presented appear a bit too cursory. The reiteration of the summary statistics is fine but the analyses seems very first order at best. I was expecting a somewhat deeper set of analyses beyond reporting of means. Results of the statistical tests for means are presented, many of which are non-significant. However the conclusions of the paper rest strongly on patterns of medians and to some extent the percentage of systems with negative trends, for which no test of significance are presented. This lack is natural flag and makes it hard to evaluate the conclusions of the paper.

RESPONSE. We spent a great deal of efforts addressing this comment. The revised version now includes statistical comparisons of medians and of percentages. Thanks for steering us into this direction.

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RC. Section 3.2 – Instead of simply noting the number of stations near islands, a more direct approach would be to calculate the trends with and without those stations. The assumption that because one subset of stations are small in numbers, they will have no significant influence on the overall pattern is appealing but strikes me as susceptible to error. If the bulk of the data have zero mean trend, the inclusion of a subset of data where trends are strong and significant can certainly sway the outcome of the summary statistics.

RESPONSE. In retrospect, the “Islands” column in Table 4 was superfluous. Several stations that are close to the continent (e.g. CalCOFI) can also be very little affected by river plumes. Given this, we removed the “Islands” column from Table 4 and also removed the two associated sentences from the original text.

RC. I find the fact that oxygen declines are detected at the surface and at depth to be quite interesting. For the coastal band, I would particularly like to hear an interpretation of this pattern with respect to the eutrophication hypothesis. One alternative of course is that physical changes (e.g. solubility) are driving the observed changes. It would be informative to see the data in terms of changes in AOU and not simply changes in DO to isolate the effects of physics from biology.

RESPONSE. As pointed out above, many of our oxygen data were lacking concomitant temperature and salinity data from which we could derive percentage of solubility. In principle, a solubility analysis or an AOU analysis would be possible, but we feel this is beyond the scope of the present paper and we defer this to a subsequent study. In the revised discussion, we now raise the possibility that physical changes, e.g. global warming in the upper ocean (Levitus 2009) may be driving the observed changes in oxygen concentration.

RC. Section 4. –Again, I am concerned that one of the core conclusions of the paper regarding difference between published and randomly compiled trends are based on a look at the median values and not on any statistical test of whether medians actually

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differ.

RESPONSE. We performed a Mann-Whitney U-test for comparing medians. The difference between published and randomly compiled trends is actually limited to the open ocean (> 100 km from the coast).

RC. I am unaware of any substantial decadal oxygen variability that is associated with ENSO variability (typically an intra-decadal or inter-annual forcing). There is good literature on ENSO effects on such high frequency variability but that ENSO variability affects the resolution of the multi-decadal scale analyses presented here would be noteworthy. Can the citation to that fact be provided? Similarly, I am unaware of any published work that links PDO to decadal variations in oxygen. I understand that Garcia et al's analyses highlight the dynamic nature of oceanic oxygen content, but it would be an important extension to conclude that the changes are known to be driven by ENSO, NAO, or PDO forcing. Also, given the similarity in data between this paper and Garcia et al's work, some discussion of how the two works agree or disagree and what is new would be informative.

RESPONSE. We clarified our sentence about the role of ENSO in driving oxygen variability on 2 to 8 years timescales (Gutierrez et al. 2008, Verdy et al. 2007). We also provided more relevant references describing the role of the NAO in driving decadal oxygen variability in the North Atlantic (Johnson and Gruber 2007), the role of the AO in driving interdecadal oxygen variability in the Japan Sea (Cui and Senyu 2010), and the role of the PDO in driving decadal oxygen variability in the North Pacific subpolar gyre (Frolicher et al. 2009). We also gave more details about the Garcia et al. (2005) findings.

RC. Table 2 –can the mean DO value from each time series be added? It would be informative to see how the stations are distributed in their mean values. Also, can the systems where H<sub>2</sub>S corrections were applied be noted? I can't tell from the text if this correction was actually used in the end.

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RESPONSE. We modified Table 2 by adding the mean DO from each time series, except for five time series that were plotted as “anomalies” in the original papers and whose mean was thus zero. In addition, time series where H<sub>2</sub>S corrections (negative oxygen) were applied are indicated by an asterisk in the mean DO column of Table 2.

RC. Tables 4 to 6 – are these tables referring only to the global dataset and not from published papers? I am expecting a direct comparison between the two sources akin to Table 3.

RESPONSE. Thank you for making this suggestion. We produced two new Tables that, in the words of referee 2, allow a more direct “apples to apples” comparison. The new Table 4 presents trend statistics from published time series, with the trends estimated over the 1976-2000 period (as in new Tables 5 to 7) rather than over the entire length of the time series. The new Table 8 presents information that formerly appeared in the last rows of old Tables 4, 5 and 6 from the BGD paper. Its results can be directly compared with those of the new Table 4.

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