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Biogeosciences Discussions

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

Anonymous Referee #1

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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. 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 enhance 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 complied. I look forward to the seeing the mature set of analyses that will make this a particularly noteworthy contribution

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

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

I wonder if the possibility of introducing artificial jumps in the time series are indeed avoided altogether. Various aspects of the winkler method has 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.

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 seasonal current shifts (e.g. eastern boundary current systems) are dominant hydrographic features, this would not be the most conservation assumption and would conceivably introduce a systematic bias in the ability to detect trends in deeper, offshore stations. This matter 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.

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

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. Also, I presume that that all stations with H2S data were eliminated from the data set. If so, why the tenuous argument about using negative oxygen values and H2S conversion in the previous section?

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 in, 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 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.).

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.

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.

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.

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 differ.

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.

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 H2S correction were applied be noted? I can't tell from the text if this correction was actually used in the end.

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.

Interactive comment on Biogeosciences Discuss., 6, 9127, 2009.