

# ***Interactive comment on “Recent decline of the Black Sea oxygen inventory” by A. Capet et al.***

**Anonymous Referee #2**

Received and published: 28 November 2015

## 1. SUMMARY

This paper aims to reassess estimates of trends oxygen content in the Baltic taking into accounts the past 60 years, split into periods of different physical and biological dynamics. The authors interpolate data using an interpolation scheme which attempts to account for variable data density in the hopes of being able to compare more data-sparse periods to the rest of the dataset. Overall, I agree fully with and would reinforce the comments made by S. Konovalov (C7404–C7407, 5/11/2015).

There is what I consider to be a significant flaw with the paper in that they base the bulk of their conclusions on a severely under sampled time-period. The interpolation that the authors perform for the majority of the basin between 1999-2013 (and to a lesser extent, 1986-1998) is difficult to trust due to the paucity of data coverage. Even the

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best interpolation scheme in the world is only as good as the input data. I'm also left wondering how sensitive the analysis is to changes in selected oxygen threshold of 20  $\mu\text{M}$ . The latter will greatly impact oxygen penetration depth estimates as the oxycline not only experiences vertical migration but also strong changes in gradient over the past 50 years.

The paper presents interesting results and a novel approach to estimating the variability of the Baltic oxygen content, but the authors need to do more to convince the reader that their study is robust due to the severe lack of data between 1993-2003. Is their method still functional in this context? Much more information needs to be provided on the results of the DIVA analysis for the reader to not dismiss the work as suffering from the issues described above.

I personally have no issue with the inclusion of ARGO data, although the authors should make a statement reminding the reader of the possible accuracy/precision issues inherent to ARGO float oxygen measurements; but agree that a more in-depth study of patchiness is necessary. I suspect there is sufficient data available from the winklers to build empirical variograms and identify scales of variability. The authors present some good figures, but need more attention to detail in the axis, labels and captions. Many captions would benefit from being fleshed out. I would also consider adding an additional figures; a diagram indicating the relative depth of the surface, bottom and CIL water masses, with a mean oxygen, H<sub>2</sub>S and either T&S or density profiles overlaid. I leave this to the author's discretion whether they feel it is necessary or not, but I believe it would complement the introduction well for readers less acquainted with the Baltic region.

Although the abstract sounds a bit stilted (I would suggest reworking it very slightly for better legibility), the rest of the manuscript reads well. The introduction is excellent, and covers the topic well. The methods section relating to the DIVA analysis must be expanded to reassure the reader that the method can cope with the huge variability of data density. The results section is brief, but to the point and highlights the important

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12, C8071–C8074, 2015

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aspects, but again I would add a section providing technical results from the DIVA analysis (assessment of variability, variability of trends identified. The discussion feels rushed; this does not impact the quality of the conclusion, rather it is my opinion that the reader would benefit from being guided through the logic and argument a bit more, particularly when relating conclusions in text to Figure 5. Finally, the conclusions were surprisingly disconnected from the rest of the paper: the last paragraph seems to bear little relation to the actual results or conclusions.

## 2. MINOR COMMENTS

16238/8: originated -> originating

16238/8: went drifting -> drifted

16239 and onwards: climatology cannot be detrended. Please correct the language throughout and provide a better explanation of what you mean.

16240/13: the spatial variability needs further explaining; I feel at the moment there is insufficient information to fully understand what the authors are saying.

16242/1-5: I'm struggling to follow the logic, please detail further.

16242/6-8: What is the importance of solubility in this analysis? Does the same trend show in % saturation?

16244/1: arose -> arise

## 3. FIGURES

Figure 1 caption could do with more details, mainly repeating the source and criteria for the profiles being kept so that it can stand independently.

Figure 2: Please expand axis labels to full words.

Figure 3 caption also needs rephrasing. For example, what trends were removed (instead of saying simply detrended). The oxygen threshold needs to be stated. Also,

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if I understand correctly “oxygen penetration density anomaly” is incorrect; it’s not an anomaly but rather the “oxygen density penetration” or “mean density at the upper oxic boundary”? Units should be written correction (kg m<sup>-3</sup>, rather than kg/m<sup>3</sup>). Also... how can climatology be detrended?

Figure 4: units need to be described correctly for each linear trend: decades<sup>-1</sup> needs to be added for each. This isn’t a nature paper, you have the space now. Units should be written correction (kg m<sup>-3</sup>, rather than kg/m<sup>3</sup>).

Figure 5: Units should be written correction (mol m<sup>-2</sup>, rather than mol/m<sup>2</sup>). I would say “Frequency distribution” rather than “Distribution density” to avoid confusion with physical density and, in my opinion, the term is more accurate.

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Interactive comment on Biogeosciences Discuss., 12, 16233, 2015.

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