Biogeosciences Discuss., 12, C7404–C7407, 2015
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Biogeosciences
Discussions
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Interactive comment on "Recent decline of the Black Sea oxygen inventory" *by* A. Capet et al.

S.K. Konovalov (Referee)

sergey_konovalov@yahoo.com

Received and published: 5 November 2015

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General comments

This manuscript is addressed to an extremely important issue of decline in the oxygen inventory in marine systems. This decline has been traced in many marine systems, but it is crucially important for oxygen deprived oxic/anoxic marine systems, like the Black Sea, for example. Indeed, the thickness of oxygenated waters in the Black Sea does not exceed upper 200 meters. Thus, even minor variations in the distribution of oxygen are important for this marine system. For all these reasons, the manuscript

suggests valuable information and it is worth publishing in Biogeosciences.

The authors analyze data from 1955 to 2014. They split all these data in several individual periods of specific trophic- and/or climate-driven changes in the Black Sea. Except for the most recent period of 1999-2013, and specifically for the period after 2010, all results and conclusions look good and well-justified.

The major problem is in DIVA analysis of highly limited and spatially located data in 1999-2013. While DIVA analysis is explained briefly for this major tool of this work, any kind of interpolation cannot fill spatial gaps of about 80-90% of the basin area (Fig. 2, lower panel). This problem seems even more serious, when DIVA analysis is applied to the position of 20 μM of oxygen, while the authors suggest that it varies versus depth and density. It is absolutely important to show that DIVA analyses is correct when it is applied to highly limited and spatially irregular distribution of data in 1999-2013.

Another problem is that the major part of observational oxygen data are from Winkler titration of water from Niskin samplers, while data for 2012-2013 are from Argo floats. I do support Argo floats, but the authors have to demonstrate that these two types of oxygen data are precisely comparable. I know, for example, that Winkler titration data for 2013 reveal sigma-t = 15.60-15.65 for 20 μM of oxygen and a rather isopycnal spatial distribution (look for the attached figure), while the authors suggest about 15.40 and a spatially variable distribution. I recommend an in-depth analysis of that patchiness in Fig. 3c and data for 2012-2013.

Specific comment.

Title. The discussed decline is not that "recent". I would suggest to drop "recent" and to limit to "Decline of the Black Sea oxygen inventory".

Page 16235, line 5. Consider "the surface layer of a lower salinity"

Page 16235, line 9. Murray et al. (1989) considered 10 μM of oxygen and the first appearance of sulfide because they analyzed high quality oxygen data from the KNORR

cruise. 20 μM of oxygen were applied later to analyze historical oxygen data of lower quality.

Page 16239. A better description of DIVA analysis is needed. What are the trends in original data? What is "detrended" spatial climatology? If a spatial climatology is applied to every specific year, it is hardly correct for both depth and density data.

Page 16240, line 2. Are these spatial variations? What are trends?

Page 16240, line 17. I would discuss a decline in oxygen penetration depth for a period, rather than an average rate because it definitely varies in time (Fig. 4).

Page 16244, line 14. It does not illustrate any decoupling because it is not discussed and/or analyzed in this work.

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

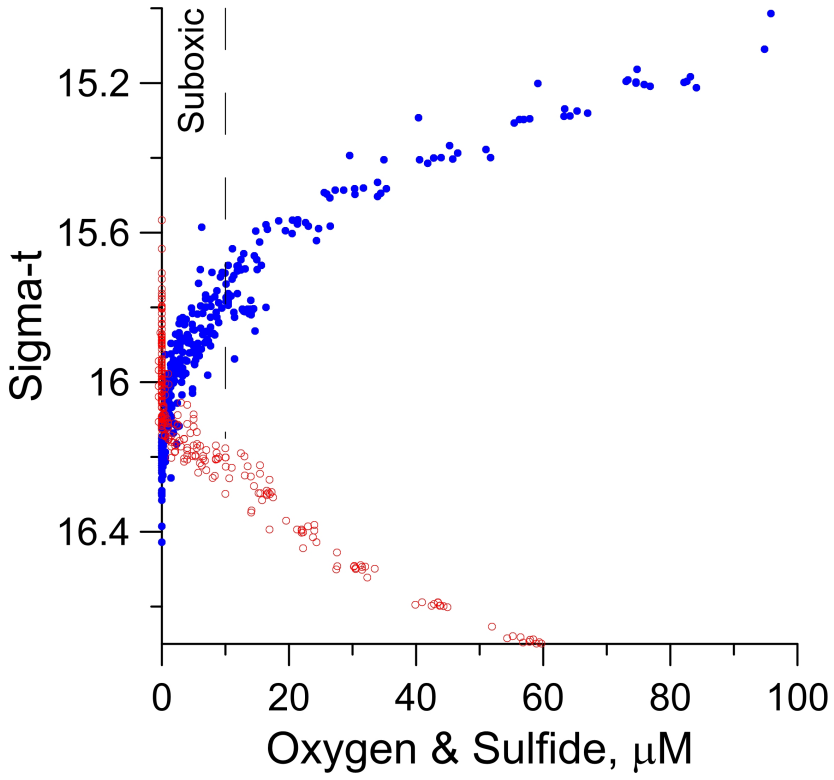

Fig. 1.