

## ***Interactive comment on “Effects of fluctuating hypoxia on benthic oxygen consumption in the Black Sea (Crimean Shelf)” by A. Lichtschlag et al.***

**Anonymous Referee #2**

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Major aim of the study presented in this manuscript was to investigate the affect of stable vs. variable bottom water levels of oxygen on benthic oxygen uptake and biogeochemical processes as well as on the macro/meiobenthic community composition and distribution at the Crimean shelf. This study thus addresses a timely scientific topic relevant to a broad marine scientific community. The study is well within the scope of Biogeosciences, which already published a range of different papers in this field.

The manuscript presents quite a diverse and extended data set on benthic biogeochemistry and macro/meiofaunal ecology. The methods with particular regard to the in situ measurements are state of the art or even cutting edge, unfortunately, only available to limited scientific community. The presented results substantially contribute to expand existing knowledge in this field.

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Overall the paper is very well written, clearly structured and the results are presented clearly.

Nevertheless, there are a few minor aspects that I would like raise:

1. Given the broad and diverse results, I somehow missed a clear take home message. Hence I recommend to add a conclusion section, briefly stating/summarizing the major findings and possible implications. The major findings should be also clearly outlined in the abstract.
2. I suggest slightly modifying the introduction. It addresses different aspects such as environmental O<sub>2</sub> threshold levels of faunal activity, different pathways of oxygen consumption or the effect of duration and frequency of oxygen fluctuations. To my feeling it is somehow difficult to understand what is really addressed here. Hence I would wish that the different aspects are tied together better with a clear orientation towards the actual aim of the study.
3. Regarding the discussion section 4.1 I agree with the comment of another anonymous reviewer that DIC measurements in the benthic chambers especially at the hypoxic environments would have been indeed helped to better constrain pathways of aerobic and anaerobic carbon degradation. Within this context, denitrification as a major anaerobic carbon degradation pathway was not addressed. This would have strengthened the study, but I still think that the data-base is sufficient to arrive at the conclusions presented here. Perhaps, the authors possess data on total alkalinity and pH in water samples retrieved from the chamber, which allow the authors to calculate organic matter degradation and comparing these rates with those measured via the TOU.
4. In the second part of the discussion section (page 6467 line 28) the discussion remains a bit vague. There is a bunch of literature addressing the topic of organism distribution at boundaries of oxygen depleted environments (e.g. Levin et al.). E.g. at the Peruvian OMZ massive macrofauna/epifauna accumulation at the lower boundary of the OMZ coined “edge effects” were observed. In most studies these effects were related to physiological oxygen thresholds as in the present study and the organic matter availability close to the anoxic boundary. These threshold values however appear to vary between the different regions suggesting that

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other factor beside oxygen might be important. Other studies (e.g. Mosch et al. 2012 Deep-Sea Research I 68, and references therein) introduce the concept of internal waves controlling deposition and suspension of particulate organic carbon, which sustain different feeding guilds and therewith control their distribution along oxic-anoxic interfaces rather than oxygen (as long as O<sub>2</sub> is present). It would have been nice if the authors could have considered such concepts as well. 5. Overall, I suggest to discuss the findings of this study a bit more in the context of other studies from world wide OMZs. 6. Just as a minor comment, since meiofauna was addressed in this study but is very often neglected it would be interesting if the contribution of the meiofauna assemblages (or only nematodes) at the different stations to the oxygen consumption could be provided by e.g. using the approach of Mahaut et al. (1995), which relates the individual respiration rate  $R$  ( $d^{-1}$ ) to the mean individual weight  $W$  (mg C) of meiofaunal organisms. (Mahaut ML, Sibuet M, Shirayama Y (1995) Weight dependent respiration rates in deep-sea organisms. Deep-Sea Res I 42:1575–1582)

#### Minor comments

Page 6447, line 8: “decreased from  $> 15 \text{ mmol m}^{-2} \text{ d}^{-1}$  in the oxic zone to  $< 9 \text{ mmol m}^{-2} \text{ d}^{-1}$  in the hypoxic zone” what does  $> 15$  mean – here I would rather expect the total range i.e. minimum and maximum.

Page 6447, line 11: “Benthic diffusive oxygen uptake rates, comprising microbial respiration plus reoxidation of inorganic products, . . .” true, but it also comprises the oxygen uptake of meiofauna, or protozoans

2.2 Faunal analyses: did you really use distilled water to wash out the meiofauna, does this not affect these organisms, especially the soft-bodied meiofauna?

2.2 Faunal analyses: I assume that sorting was conducted under a binocular rather than a compound microscope, could you provide the magnification, which was used for sorting

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2.2 Faunal analyses: what you mean with the statement macrofauna was qualitatively assessed, could you please better specify how the analysis of macrofauna was conducted?

Page 6454, line 25 “Oxygen concentrations in the chamber was the same as in situ bottom water concentrations.” Don’t understand this sentence, do you mean that at the start of the incubation the O<sub>2</sub> level inside the chamber was the same as measured outside?

Page 6455, line 4 “To estimate the in situ ratio of TOU/DOU for the hypoxic-anoxic zone, we modeled the DOU based on the volumetric rate and the DBL thickness determined by the in situ microsensors profile” What do you mean here with “modeled”? Higher up you mention that DOU was calculated.

Page 6457, line 19 “During our sampling campaign the horizontal distance to the oxic-anoxic interface (chemocline) was on average 13km.” I think it would help if the location of the oxic-anoxic interface could be denoted in Figure 2 (and probably Fig. 1).

Page 6457, line 22: “. . . Fig.6 .. “ suggest to number the figure in order of their appearance in the text.

Page 6460, line 19: “Highest fluxes in the oxic-hypoxic zone, however, were not recorded during a “normoxic event” ( $149 \mu\text{mol O}_2 \text{ L}^{-1}$ ), but at the typical intermediate bottom water oxygen concentration of approx.  $90 \mu\text{mol L}^{-1}$  (Fig. 4b and c, Fig. S1b).” This statement is not consistent with Fig. 4b, which shows bottom water levels of  $140 \mu\text{M}$ .

Page 6462, line 19: “. . . takes place below the oxygenated sediment . . .” please reformulate to “. . . oxygenated sediment surface . . .”

Page 6464, line 27: would be nice if authors could provide data confirming the accumulation of organic matter in the sediments at the onset of hypoxia.

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