Answer to Anonymous Referee #2

We are grateful for Anonymous Referee #2's comments. We have followed all suggestions, and we believe that the revised version of the manuscript is now much improved. The manuscript has been modified and will be uploaded when required from the editorial system. Below you can find our response to the general and specific comments marked in italics.

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.
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:

Referee #2:
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.
REPLY: *The authors have included a conclusion section at the end of the manuscript.*

Referee #2
2. I suggest slightly modifying the introduction. It addresses different aspects such as environmental O2 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.
REPLY: The authors have shortened and revised the introduction accordingly and specified the aim of the study more clearly.

Referee #2
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.
REPLY: Similar to the answer to the comments from anonymous referee #1, we agree with the reviewer that DIC flux measurements in the chamber would have been a helpful addition to this manuscript. Originally we had aimed to measure the DIC fluxes in the chamber (and thus did not sample for pH and total alkalinity), however, using flow injection measurements and having a relatively small volume sample for DIC measurements left from the chamber samples, we found the results from our DIC measurements not accurate enough to reliably determine the carbon flux rates. However, we are glad that reviewer #2 agrees with us, that the data is still sufficient to deduce the presented conclusions. We added a statement saying that denitrification plays most likely a minor role in our study and now included the nitrate concentrations in the Methods/Results section. We hope that we have communicated clearly in the paper that the main focus was on oxygen respiration rates, as we were not equipped to get the full in situ element fluxes covered.

Referee #2

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 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 O2 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.
REPLY: *We have now included a short discussion paragraph (in 4.2 Effect of bottom water fluctuations on faunal respiration and diffusive oxygen uptake) to point to the differences between the different regions, and have added the suggested reference. Also, we have discussed that sediment accumulation did not vary much according to our measurements (P6463 L 4), hence we may have another situation as in the earlier studies.*

Referee #2
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)
REPLY: We agree with the reviewer that this would be really interesting. However, we have data on meiofauna weight only (and partially) from one station of the hypoxic-anoxic zone. Due to this very limited dataset as well the uncertainties of microbial vs meiofauna respiration under nearly anoxic conditions, we think that our data are not good enough to attempt this.

Minor comments:

Referee #2: Page 6447, line 8: "decreased from > 15 mmol m−2 d−1 in the oxic zone to < 9 mmol m−2 d−1 in the hypoxic zone" what does > 15 mean – here I would rather expect the total range i.e. minimum and maximum.
REPLY*: We change this now to "on average 15 mmol* m^{-2} d^{-1} *in the oxic zone to on average 7 mmol* m^{-2} d^{-1} *in the hypoxic zone". As we discuss everything in respect to different zones, we think the paper benefits more to give in this case the average values of the different zones than absolute minima/maxima.*

Referee #2: 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
REPLY*: We amended the sentence and included a statement that diffusive oxygen uptake rates also includes oxygen uptake by small eukaryotes including protozoa and smaller meiofauna.*

Referee #2: 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?
REPLY: Yes, we used filtered or distilled water. This method is used for more extreme conditions, e.g. in sulfidic zones, to prevent the introduction of animals from oxic waters above. Distilled water does not affect the morphological structure of the pre-fixed meiofauna, including soft-bodied fauna.

Referee #2: 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
REPLY: We used a binocular compound microscope (Olympus CX41, 90-folds magnification). We now specified this in the text.

Referee #2: 2.2 Faunal analyses: what you mean with the statement macrofauna was qualitatively assessed, could you please better specify how the anaylsis of macrofauna was conducted?

***REPLY**: This was done similarly as with the meiofauna, by counting them and identifying them to higher taxa. We add this now in the text: "In the same cores we did quantitative analyses of fauna that from their linear dimension (> 2 mm) are included in the macrobenthos. This share of the fauna was identified to higher taxa under the microscope, counted and the abundances were extrapolated to m^2."*

Referee #2: Page 6454, line 25 "Oxygen concentrations in the chamber was the same as in in situ bottom water concentrations." Don't understand this sentence, do you mean that at the start of the incubation the O2 level inside the chamber was the same as measured outside?

***REPLY**: Yes, that is what we mean. We rephrase the sentence now to " At the beginning of the incubation period, oxygen concentrations in the chamber were the same as in situ bottom water concentrations outside the chamber".*

Referee #2: 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 microsensor profile" What do you mean here with "modeled"? Higher up you mention that DOU was calculated.

***REPLY:** That is right that we usually calculated the DOU, however, as the TOU measurements in the hypoxic-anoxic zone failed and we wanted to assess the TOU/DOU ratio at this specific oxygen concentrations, for this case we modeled the DOU from the volumetric rates and the DBL thickness. To make this clearer, we reformulated the sentence to "To estimate the in situ TOU/DOU ratio for the hypoxic-anoxic zone, in this case we modeled the DOU at these specific conditions based on the volumetric rate and the DBL thickness determined by the in situ microsensor profile" .*

Referee #2: Page 6457, line 19 "During our sampling campaign the horizontal distance to the oxicanoxic 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).

***REPLY**: In this study the oxic-anoxic interface was not a sharp boundary but fluctuating by tides and internal waves, as we have discussed (chapter 3.1 Oxygen regime of the outer Western Crimean Shelf and P 6463 L 25 to P6464 L8). Measurements indicate that the oxic-anoxic interface spreads over a wide area. In principle, the whole oxic-anoxic zone "is the chemocline". For this reason we would prefer to keep it rather as "zone" as we will not be able to report a specific depth as chemocline. The zones are already clearly indicated both in Fig. 1 and 2.*

Referee #2: Page 6457, line 22: ". . . Fig.6 .. " suggest to number the figure in order of their appearance in the text.

***REPLY**: Yes, we agree, we changed the numbers of the figure now in order of their appearance.*

Referee #2: Page 6460, line 19: "Highest fluxes in the oxic-hypoxic zone, however, were not recorded during a "normoxic event" (149 µmol O2 L−1), but at the typical intermediate bottom water oxygen concentration of approx. 90 µmol L−1 (Fig. 4b and c, Fig. S1b)." This statement is not consistent with Fig. 4b, which shows bottom water levels of 140 µM.

***REPLY**: We agree that the labelling of the panels might be misleading in this case and corrected this to "Highest fluxes in the oxic-hypoxic zone, however, were not recorded during a "normoxic event" (144 µmol O2 L−1, Fig. 4b), but at the typical intermediate bottom water oxygen concentration of approx. 90 µmol L−1 (Station 434; Fig. 4c, Fig. S1b)."*

Referee #2: Page 6462, line 19: ". . . takes place below the oxygenated sediment . . ." please reformulate to ". . .. oxygenated sediment surface . . . "

***REPLY**: In this case we do mean "below the oxygenated sediment", as the sediment surface would be the sediment/water interface. Sulfate reduction only takes place when no dissolved oxygen is left, which for this case corresponds to sediments below approx. 1 cm.*