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Biogeosciences Discussions

Interactive comment on "Oxygen penetration deep into the sediment of the South Pacific gyre" *by* J. P. Fischer et al.

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Final response to Referee 1 (anonymus)
We thank referee 1 for his/her comments and the opportunity to clarify certain misunderstandings. Answers to the referee's specific comments are reported point by point. Changes in the text are located by the number of the corresponding line in the original manuscript.

As the reviewer points out, our manuscript represents a novel data set on oxygen profiles in oligotrophic sediments. This is true not only for the deep penetration of oxygen, but also for oxygen flux measurements in the southern Pacific Ocean. There are data available from the central Pacific (mentioned by reviewer 2 and now incorporated in the discussion) and other oligotrophic sites but those data are still very limited. Benthic

oxygen fluxes provide a good and integrated measurement of the metabolic activity of surface sediments. They quantify benthic carbon mineralization rates and thus can be used to evaluate the efficiency of the biological pump (export of organic carbon from the photic zone). We believe that our data can contribute to the regional understanding of the marine carbon cycle in oligotrophic regions and provide valuable information for the global carbon cycle. Such regions are still undersampled (e.g. Jahnke 1996).

General comments:
The reviewer finds the number of visited / evaluated sites insufficient and doubts, that the sites represent a cross section through the gyre. Furthermore, he / she would like to see replicates for the measurements / samples. His / her main criticism is our comparison of fluxes of organic carbon, derived from DOU measurements with results from remote sensing primary production data for the same region. Especially the fact that we find a lateral gradient in our data for the northern stations, which is less pronounced in the PP-derived measurements, leads the reviewer to the conclusions, that these datasets should not be compared.

Reply: It is the nature of deep sea research that it is often constrained by low numbers of samples and replicates. The R/V Revelle Knox-02RR Expedition, whose principal objective was to gain geophysical, sedimentological, geochemical and microbiological data in support of an IODP drilling proposal, traversed an area of the ocean greater in extent than that of North America over the course of 40 days. The focus of the coring operations was on obtaining deep sub-seafloor samples (piston-cores). Water depths of up to 5700 meter, weather, and the technical challenges of open ocean research meant that only one multi-core sampling per station was allotted. Of course, it is always desirable to have more sampling stations, covering an even larger area. The 11 stations visited during this 6 week cruise were chosen to reflect two transects, spanning from the rim to the center of the gyre. We believe that this goal was accomplished, since we covered a very large area of crust age, sediment coverage

and also surface chlorophyll values.
We present the deepest oxygen penetrations found so far, accompanied with in-situ and ex-situ microsensor profiles of the top layer used to quantify benthic carbon mineralization of a so far unexplored region in this respect. Benthic oxygen fluxes are a good measure for the organic matter mineralization of deep-sea sediments as they are direct measures of benthic activities. In contrast, carbon fluxes derived from surface primary production and water depth are based on empirical relations. Comparisons between primary production, sediment trap analyses and benthic oxygen fluxes have, to our knowledge, always shown a mismatch in estimated carbon mineralization rates (e.g. Gehlen et al. 2006). With this background, the differences in the carbon mineralization rates derived from remote sensing and empirical relations and our oxygen microprofile derived fluxes appear small. This has been pointed out more clearly in the revised manuscript and possible reasons for the remaining mismatch are discussed in detail.
It appears that the reviewer got the impression that the microsensor data presented for 5 stations were picked out of a total number of 11. We clarified in the revised manuscript that this is not the case and no measurements were left out for the publication.

Specific comments:
P3160, L4 "most oligotrophic": word choice is awkward
R: Word changed - extremely oligotrophic.

addition of average C mineralization rate to the abstract
R: The carbon mineralization rate of course changes with depth. End member values have been added to the abstract.

P3161, L22 use of the reference D'Hondt et al. 2009 is difficult to evaluate, since the paper is unpublished and un-reviewed. It is also difficult to evaluate, which data is repititous to D'Hondt et al. 2009

R: In the meantime, D'Hondt et al. is in press and we expect it to be publicly available soon.

P3161, L24: "subseaflor"
R: word was corrected

P3161, L26: "focusses"
R: word was corrected

P3162, L8: "metres"
R: word was corrected

P3162, L16: What is the number of cores taken at each station? If n=1, comparisons across the cruises large area are difficult to evaluate
R: At each station, two cores were taken, while only one was used for the determination of oxygen profiles (see comment above).

P3163, L20: "in some cores": please specify
R: The requested information was added to the manuscript

P3163, L18 why were only 5 cores evaluated? Where the other results excluded?
R: The results were not excluded from the analysis. Due to technical limitations, it was only possible to measure microelectrode profiles in cores from 4 out of 11 stations. Additionally, 1 in situ profile could be obtained. All these profiles are shown in the manuscript. These facts are clarified in the text.

P3164, L3 Remove comma after "both"
R: The comma clarifies that "both" refers to piston cores and trigger cores. Eliminating the comma would allow the interpretation, that two piston cores were measured.

P3164, L3 Why were the (piston) cores incubated at 20°C and not at 4°C as for the microsensor measurements? This may cause an issue for comparing and combining of the measurements and might change the O2 concentrations. Please explain and validate
R: The raised temperature decreases the solubility of oxygen within the porewater of the sediment. If supersaturation was reached, a change in oxygen concentration would have been the result. However, the oxygen solubility at 20°C and salinity of 35 is 230 μmol L-1. This is below the bottom water concentration of all our sides. Therefore, oversaturation could not occur. Since the volumetric respiration rates in the deeper layers are very low, a small variation in this rate due to warming would not have affected our measurements on the time scales involved. The methods section of the revised manuscript was extended for this rationale.

P3165 - Modeling: Why do the authors make their own model? Why not use e.g. the well cited and robust model of Berg. et al 98)?
R: The model we used is the general 1-dimensional diffusion equation with respiration terms. We chose to implement the model ourselves to have full control over boundary conditions etc. Additionally, since the spatial scale in the top layer (100μm steps) is completely different from the deeper layers (100mm steps), special means have to be taken to ensure numerical stability. In the revised version of the manuscript, the modeling was done analytically. As suggested by reviewer 2 we now choose a common model formulation for all our modeling. Different parameterizations have been chosen to account for the different aspects of the data (e.g. calibration of the model with the piston core oxygen data, extrapolation and incorporation of the high

resolution surface layer. The modeling section in the revised manuscript should thus be much clearer now.

P3166, L15 "excact"
R: word was corrected

P3168, L1 The authors state the exclusion of advection, but do not provide data or references for this fact.
R: Oceanic red clays with a grain size of few microns do not allow for any substantial porewater flow (e.g. Spinelli et al. 2004). The reference has been added to the manuscript.

P3168, L8 Why were only some of the cores evaluated for microsensor profiles and why were some done under in-situ conditions, while others were done under ex-situ conditions?
R: Technical problems allowed us only to obtain one in situ microprofile. Also the number of multi-cores available for ex-situ oxygen measurements was limited.

P3168, L8 I am concerned about the ex-situ / in-situ comparison, as the in-situ profile (station 10) produced a much larger uptake, compared to the ex-situ profiles.
R: This point was included in the discussion of the revised manuscript. The in-situ uptake rate at station 10 is indeed about twice as high as the ex-situ rates measured at the other stations. However, since the surface PP is also considerably greater at this station, the result is not unlikely. In sediments with low overall reaction rates, the differences between ex-situ and in-situ measurements tend to be smaller than in highly reactive sediments. Therefore, we believe that the comparison of the in-situ profiles with the ex-situ measurements is justified.

P3168, L8 How can sampling artifacts be exempted between the different conditions. The ex situ profiles (except for station 5) look much more erratic than the clean in-situ profile at station 10.
R: The ex-situ profiles at station 4 and 6 indeed show a lower signal to noise value than the profiles at station 5, 7 and 10. The reason is not the difference between in-situ or ex-situ conditions, but the fact that different microsensors have different qualities. This does not strongly affect the general results.

P3169, L10-20 I generally disagree with the author's comparisons of their data to those calculated from PP data. The measured profile data shows a decreasing Jpoc across sites 4-7, while the PP data does not; also the variations between sites 4-7 are 50 and 12%, respectively for each method. From the PP data, sites 1-7 (the full width across the gyre) show no trend of decreasing Jpoc across the gyre, which in general disputes their conclusion that DOU decreases across the gyre. The PP data also shows that sites 9-12 are generally much different than sites 1-7, and I would suggest that something other than the authors' conclusions that DOU decreases across the gyre is occurring. This issue is complicated by the fact that the authors do not have this data from many of the sites (stations 1-3, 9, 11-12). This argument represents a major problem for the authors' main conclusion that DOU decreases across the gyre, as well as being an invalid comparison across these data sets.
R: We disagree with the reviewer since we believe that our DOU derived carbon fluxes represent 3 different primary production regions (indicated as mean Chl a in Fig. 1 in the revised manuscript). As stated earlier oxygen fluxes provide an integrated measurement of the metabolic activity of sediments and thus carbon mineralization. Our benthic C-fluxes generally mirror the primary production pattern of the investigated region. The fact that measured fluxes and fluxes derived from empirical relations do not match perfectly, points more to the fact that the link between ocean productivity

and particle export is less well understood, rather then one estimation is better then the other. We discussed in detail problems, arising from the interpretation of ocean color data for JPOC measurements, especially for ultra-oligotrophic areas for which the models/empirical relations are poorly calibrated. The reviewer implies that calculations of organic carbon fluxes to the seafloor derived from remote sensing ocean color data coupled to an uncertain sedimentation and watercolumn recycling model give more reliable results than direct measurements of oxygen fluxes on retrieved cores. This seems not very likely to us. Furthermore, it is not the main conclusion of the manuscript that DOU decreases across the gyre; it is not even mentioned in the conclusion section. We feel that the reviewer misinterpreted this part of the manuscript and therefore, we tried to further clarify the issue in the revised manuscript.

P3170, L13 sentence confusing, specifically: "showed an initial drop in concentration comparable to the decrease in the deep profiles, but within the first few centimeters" - how is this comparable?
R: We agree with the reviewer about the confusing sentence and it was changed in the revised manuscript. We wanted to express, that the drop in concentration observed in the deep profiles over 1 m actually happens within a few centimeters, as shown by the microprofiles. This initial drop seems to be smeared in piston cores by the coring procedure.

P3170, L27 "was modeled exemplary" - I am not sure, what this means
R: Originally, we modeled only one site in detail in order to keep the manuscript short. However, since both reviewers suggest to model all sites were surface and subsurface data are available we included these results in the revised manuscript.

P3171, L2 "beeing"
R: word was corrected

P3172, L14-15: "sediments from station 1-11 are geochemically similar and microbial cell numbers are comparable" Please provide data and/or reference for this statement. This is a very broad statement that covers a huge spatial area; I would expect some variations across such a large area

R: Maybe the term "similar" is to strong in this context and the sentence has been changed accordingly. However, variations are very limited across the area. An extensive set of background data can be found in D'Hondt et al. (2009), which is cited at this position in the manuscript and which is in press now.

References

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