

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

J P Fischer

jfischer@mpi-bremen.de

Received and published: 12 June 2009

Final response to Referee 2 (F. Meysman)

We thank F. Meysman for the detailed and valuable comments, which improved our manuscript. We made a thorough revision of the paper with special focus on the modeling section and followed the reviewer's suggestions in most points. Answers to the referee are reported point by point. Changes in the text are located by the number of the corresponding line in the original manuscript.

Comments:

Missing background data on the sampling stations. In order for the O₂ flux data to

C696

be useful for the scientific community (eg when adding them to a data repository), the data needs suitable accompanying metadata for the sampling sites. In this respect, Table 1 compiles only very limited information: only water depth and sediment thickness are given. Following essential data are missing: temperature, salinity, and O₂ concentration of the bottom water, porosity information, grain size characteristics. Moreover, many times, one is referred to the D'Hondt et al (2009) paper for crucial info. I would like to see the paper stand much more on its own. Often one only needs to plug in some actual numbers into the sentences (which will not extend the paper very much).

Reply: We agree that additional metadata are helpful for the site characterization and added most suggested parameters to Tab. 1. Bottom water temperature and salinity are highly constant over the entire region (1.2-1.4°C, salinity: 34.7), as derived from the ICES database. This information has been added to the text. Bottom water oxygen was included in the table and a figure displaying depth profiles of $\phi * D_s$ as derived from conductivity measurements was included in the manuscript. Grain size characteristics were not described in detail, and seem to play a minor role for this study.

Modeling. The modeling part should be adapted and improved. I have following suggestions and remarks: (a) The model notation is taken over from Murray and Grundmanis (1980), and is laborious and obsolete. Consumption rates should not indicated by partial derivatives. Use simply capital "C" instead of "c(O₂)" to denote the concentration. Use simple "R" instead of "resp" to denote the O₂ consumption rate. (b) Three different models are presented and employed to analyze the O₂ profiles: an empirical exponential fit (eq 1), a model for the deep part of the cores (Eq 2-3), and a separate model for the top of the cores (eq5). This proliferation of models is needlessly complex. Everything can be captured by one single model (which is essentially eq 5)

$$D_s * d^2C/dz^2 - R_{surf} - R_{base} = 0$$

R_{base} denotes the O₂ consumption due to refractory org C that is present down in the core. This rate will be small. R_{surf} then represents the O₂ consumption due to fresh

C697

org C added within the top of the core. Both Rbase and Rsurf can be parameterized as either constant with depth (1 par - the simplest model) or exponentially decreasing with depth (2 pars - more complex model). The Rbase params can be calibrated on the data from the lower part of the cores. Subsequently, one can plug them into the model and treat the micro-electrode data from the upper part of the core. (c) The empirical exponential model (eq 1) is entirely superfluous since one uses the "deep" model (eqns (2)-(4)) anyway to extrapolate down the basalt. Moreover, why using an exponential depth dependence if the "deep" O2 profiles in Figs 1, 2 and 5 really look straight to me (Occam's razor: the simplest model is the best, so I would simply use linear extrapolation).

Reply: The modeling section of the manuscript has undergone a thorough revision. We followed the reviewer's suggestion to use a single model with different parameterizations for the fitting of the deep and shallow profiles as well as for extrapolation instead of using three different models. The general model now reads:

$$\phi D_s * d^2C/dz^2 - R_{surf} - R_{deep} = 0$$

where ϕ is the porosity, D_s is the sediment diffusion coefficient, including the effect of tortuosity. C is the oxygen concentration at depth z . R_{deep} is constant with depth and R_{surf} is a respiration term which is exponentially decreasing with depth. For the initial fit of the piston core data, R_{surf} was set to zero. The measured concentrations at the upper limit and the downward flux at the deepest data point were used as boundary conditions. This flux and R_{deep} were varied. The empirical exponential model has been eliminated and extrapolations were done with the calibrated model as suggested. Furthermore, analytical solutions of the 1D reaction diffusion equation have now been used instead of numerical modeling, which makes our results much more traceable.

C698

(d) The "deep" O2 model (eq 2-3) assumes a constant porosity. Is this justified in such deep cores? The formation factor F was determined via resistivity measurements (the latter are not explained in the methods).

Reply: A figure, showing the formation factor measurements from all stations has been added and the methodology was included in the methods section. We are aware, that the assumption of constant porosity below the depth we measured is carrying some uncertainties, especially for the stations with thicker sediment cover. A discussion of this issue has been added to the manuscript.

(e) Error in Eq (2). The relation between the sediment diffusion coef D_s and the formation factor F is indeed $D_s = D_0/(F*por)$. However, one should use D_s in Eq (2) and not D_0/F . (f) Error in Eq (3). The porosity is missing from the flux

Reply: The reviewer points out, that the model formulation is misleading and that it appears that porosity is missing. The formulation has been changed to clarify, that porosity is included via the use of the formation factor.

(g) The upper boundary condition for the deep O2 model is not specified.

Reply: The model formulation and implementation has been changed. The upper boundary condition is now specified.

(h) Currently, calibration and extrapolation down to the basalt are done at the same time. I would separate these two model steps. I would first fit the deep O2 model only to the zone where O2 data is available. Subsequently, in a separate section, I would discuss possibilities of extrapolating the model results down to the basalt in the zone where no data are available (either by simple linear extrapolation or using the calibrated deep O2 model). I would also more carefully discuss the uncertainty associated with such an extrapolation procedure.

Reply: We followed the reviewer's suggestion to fit the model first only to the depth,

C699

where data is available, before we extrapolate. For this purpose, the model was parameterized with constant respiration rate and fixed concentrations at the upper boundary. The downward flux at the lower boundary (deepest data point) was the second boundary condition. After determining a range of well fitting flux / respiration combinations (including zero respiration = linear fit), we used those to extrapolate to the basalt. A discussion of the uncertainties of such an extrapolation was added and the speculations about possible fluxes to the basalt are more cautious now.

(i) Why is the surface O₂ model not applied to the data profiles in fig 4?

Reply: the combined surface / deep model has now been used to fit the other microsensors profiles as well.

Respiration rates. No data is provided on fitted O₂ consumption rate parameters (resp_max, alfa, and resp_const in eqs 4 and 5). How does the best fitting value of "resp_max" in Eq (4) compare between stations? Moreover, I have trouble believing that all the combinations in Fig 7 provide a really good fit to the data. High respiration rates would provide curvature to the O₂ profiles, and such curvature appears absent to me.

Reply: The comparison between the constant deep rates was presented in Fig. 7 (original manuscript), in dependence of the used fluxes to the basalt. The surface profiles were not compared in the first version of the manuscript. This aspect has been added (s. reply "modeling, i)"). It has to be noted, that the tested respiration rates were all in the range of $\mu\text{mol m}^{-3} \text{y}^{-1}$, which is too small to result in strong curvature. Fig 6 (original manuscript) shows, that, higher respiration rates indeed result in higher curvature. However, the curvature is still small, so that the fit is reasonably well. Since the whole modeling section has been strongly improved, this issue should be clearer now.

C700

Referencing. The literature on deep oxygen penetration and the model analysis of these profiles is only partially covered. In fact, the Murray and Grundmanis (1980) paper is the main study referenced. The MANOP (MANganeses NOdule Program) project has spawned a lot of literature on the sediment geochemistry of the central pacific in the eighties. Useful references in this regard are: Hammond et al (1996) DSR (see the O₂ profile analysis in here) Jahnke et al (1994) GCA Reimers et al (1984) GCA Furthermore, other studies in the Atlantic have measured really low TOU rates (lower than the ones reported here), which then should be accompanied by deep OPDs. Smith (1978) Marine Biology Sayles et al (1994) Nature Smith (1987) LO Although it is not the literature I am not very familiar with, I suspect there is substantial work done by geophysicists on the pore water convection cells within the basalt, which "consume" the O₂ flux into the basalt. A short discussion focused on this could improve the discussion section.

Reply: The suggested literature was evaluated and citations are incorporated into the manuscript. Concerning the understanding of convection cells and flow within the basalt and underlying sediments, this is one of the great unknowns in modern oceanography, and is the focus of a number of ongoing IODP expeditions, both current and proposed. Geophysicists have traditionally relied on heat flow data, but as Rudnicki et al. (2001) point out, geochemical data appears to be a more sensitive indicator of fluid advection. Quantifying transport in cooler, sediment-basalt regimes with more subtle flow rates (i.e., the equatorial Pacific and the SPG) requires the development of biogeochemical tools (such as the use of dissolved oxygen concentrations) to probe these problems.

*Section 2.3. Calculation of Ds. It is unclear to me how the Ds is actually calculated from the transient profiles. Apparently some transient model is applied, but no model solutions are indicated in Fig 2 (provide them so we know how good the model fit). Moreover, the term "Einstein-Smoluchowski equation" is confusing as two equations are referenced like this. [1] $D = \mu u * k_b * T$ (μ viscosity, k_b Boltzmann constant, T temperature) [2] $D = L^2 / (2 * \tau)$ (L mean step length, τ waiting time between two*

C701

steps) However, I sense that none of these two is the one that is actually used.

Reply: The term Einstein-Smoluchowsky equation is commonly used in the literature to describe the relationship between time and average diffusion distance, depending on the diffusion coefficient. The formula has been added to the manuscript to avoid misunderstandings and the term was changed to Einstein-Smoluchowsky diffusion equation.

Detailed technical comments; replies are marked with 'R':

P3162.L15. To get a feel for its size, qualitatively indicate the extent of South Pacific gyre on the map.

R: Areas with chlorophyll concentration below 0.1 and 0.03mg/m³ have been added to the map to indicate the gyre

P3162, L26 use the term optode

R: The term was changed according to the suggestion

P3163.L1 This section suggests that many in situ measurements were done. However, this was only done for one site. Explain why such data are not available for the other sites.

R: Out of 4 deployments, three failed to provide useful data due to technical problems. This information has been added to the manuscript

P3163.L10 Can't really check the value of the sediment diff coeff because no salinity, depth and temp are provided. For T=2 deg C, P=400 bar, S=35, and a tortuosity of 1.2, I get a value of 7.55 E-06 cm² s⁻¹. DOU calculations not only require a sediment diff coeff value, but also a porosity. Was this accounted for in the DOU calculation and what porosity value was used?

C702

R: salinity and temperature have been added to the manuscript. Instead of using porosity explicitly to calculate the flux, we used the sediments formation factor. Since the calculation of the sediment diffusion coefficient from formation factor includes porosity in the denominator, it is canceled down in the calculation of flux.

P3163.L17 What is the actual value of the DBL thickness in situ?

R:The actual in situ DBL thickness could not be determined precisely from the microprofiles. For ex-situ measurements, we prohibited stagnant overlying water by stirring. Since the DBL thickness most likely will not be rate-limiting at the observed reaction rates, it was not necessary to mimic the in situ DBL thickness precisely.

P3165.L6 what type of model calculations?

R: We estimated the speed of a front, entering the core liner radially from outside by the Einstein-Smoluchowsky diffusion equation. This information is not relevant, since we actually measured radial profiles to ensure unaffected measurements. Therefore, the sentence has been omitted in the revised manuscript.

P3166.L10 How are r² values calculated? r² is typical a goodness-of-fit measure for linear models...

R: The values for r² were calculated as the sum of squares of the distances of the data points to the fitted model at the respective depths, normalized to the squared distances of the points to the mean of all values. Traditionally, the symbol r² is used only for linear regression while R² is used for nonlinear regressions (while the calculation stays the same). We changed the symbol therefore to R².

P3168.L11-19 Move this section to the methods section

R: The section was moved to the methods section

C703

P3168.L23. Ref is missing. Compare to other values from MANOP literature

R: The reference has been added and the manuscript has been expanded by comparisons to the suggested MANOP literature.

P3170.L5 "not exceptionally low" : with reference to what other sites?

R: The fluxes are not exceptionally low in comparison with other oligotrophic areas, (e.g. Hammond et al. 1996; Murray and Grundmanis 1980; Wenzhöfer and Glud 2002). The manuscript was changed accordingly.

P3170.L22 "robust" -> not the right word, given the high uncertainty on extrapolation. Use linear extrapolation rather than exponential.

R: In the revised version, we used a linear extrapolation and are more cautious, concerning the interpretation of the deep extrapolations.

P3170.L22 "a small flux...was present" What is meant by this, the integrated O₂ consumption rate over the deep zone?

R: although the respiration rates are very low, there is obviously a downward flux of oxygen in the order of $<1 \mu\text{mol m}^{-2} \text{d}^{-1}$.

References

Hammond, D. E., J. Mcmanus, W. M. Berelson, T. E. Kilgore, and R. H. Pope. 1996. Early diagenesis of organic material in equatorial Pacific sediments: stoichiometry and kinetics. *Deep Sea Research Part II: Topical Studies in Oceanography* 43: 1365-1412.

Murray, J. W., and V. Grundmanis. 1980. Oxygen Consumption in Pelagic Marine Sediments. *Science* 209: 1527-1530.

Rudnicki, M. D., H. Elderfield, and B. Spiro. 2001. Fractionation of sulfur isotopes during bacterial sulfate reduction in deep ocean sediments at elevated temperatures. *Geochimica et Cosmochimica Acta* 65: 777-789.

C704

Wenzhöfer, F., and R. Glud. 2002. Benthic carbon mineralization in the Atlantic: a synthesis based on in situ data from the last decade. *Deep-Sea Res. Part I Oceanogr. Res. Pap.* 49: 1255-1279.

Interactive comment on Biogeosciences Discuss., 6, 3159, 2009.

C705