**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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General evaluation:

This is the first time that the O2 concentrations were measured so deep down the cores in the oligotrophic areas of the ocean. I have always wondered what happened beneath in those sediments where the O2 did not go down to zero within measuring range. This paper resolves this mystery to a great extent, and therefore this is a crucial dataset. As a whole, this ms provides a valuable contribution to sedimentary carbon processing in these oligotrophic areas. Moreover, the paper is generally well written, has the right length, and the discussion tackles the appropriate topics. After a suitable revision, this work readily deserves publication.

Specific comments and suggestions:

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

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)

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

Rbase denotes the O2 consumption due to refractory org C that is present down in the core. This rate will be small. Rsurf then represents the O2 consumption due to fresh 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 de 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 eth 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). (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). (e)Error in Eq (2). The relation between the sediment diffusion coef Ds and the formation factor F is indeed $Ds = D0/(F*por)$. However, one should use Ds in Eq (2) and not D0/F. (f)Error in Eq (3). The porosity is missing from the flux. (g) The upper boundary condition for the deep O2 model is not 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 were 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. (i) Why is the surface O2 model not applied to the data profiles in fig 4?

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.

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 (MANganese 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) L&O 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.

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 hood good the model fist). Moreover, the term "Einstein-Smoluchowski equation" is confusing as two equations are referenced like this. [1] $D = mu*k\_b*T$ (mu viscosity, k_b Boltmann constant, T temperature) [2] $D = L^2/(2*tau)$ (L mean step length, tau waiting time between two steps) However, I sense that none of these two is the one that is actually used.

Detailed technical comments:

P3162.L15. To get a feel for its size, qualitatively indicate the extent of South Pacific gyre on the map. P3162.L26. optode sensors 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. 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 P3163.L10 DOU calculations not only require a sediment diff coeff value, but also a porosity. Was this accounted for inthe DOU calculation and what porosity value was used? P3163.L17 What is the actual value of the DBL thickness in situ? P3165.L6 What type of model calculations? P3166.L10 How are r2 values calculated? r2 is typical a goodness-of-fit maesure for linear models... P3168.L11-19.

Move this section to methods section P3168.L23. Ref is missing. Compare to other values from MANOP literature P3170.L5 "not exceptionally low" : with reference to what other sites? P3170.L22 "robust" -> not the right word, given the high uncertainty on extrapolation. Use linear extrapolation rather than exponential. P3170.L22 "a small flux...was present" What is meant by this, the integrated O2 consumption rate over the deep zone?

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