

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

Anonymous Referee #1

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Review: “Oxygen penetration deep into the sediment of the South Pacific gyre” J. P. Fischer, T. G. Ferdelman, S. D’Hondt, H. Røy, and F. Wenzhofer

The MS presents an interesting data set on the fluxes and consumption of oxygen in an oligotrophic deep sea environment. The paper presents a novel data set on the deep penetration of oxygen into sediments that have a very low productivity, due to the low availability of organic matter. While this in itself is very interesting, the interpretation of this data and the ensuing conclusions need to be addressed, as there are a number of discrepancies. Many of these discrepancies stem from an incomplete data set (e.g. 11 stations were visited, while full data are presented for only 5) as well as the fact that their sites used as a transect across the gyre is incomplete and questionable (due to anomaly of site 10, as it is further south). This in itself could make some readers

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skeptical of the conclusion made here-in, as no reason for the exclusion of the other stations is given.

I disagree with the authors comparison of their profile calculated Jpoc data with that calculated from primary production. The PP data shows little variation across the gyre, with the exception of the Southern sites 9-12, which (from the PP data) appear to be much different from the Northern sites 1-7. The authors infer a gradient of DOU across the gyre, while the PP data seems to dispute this. I find fault in the author’s comparison of these two data sets, and would suggest that the PP data not be used to support their data set in the current configuration.

Considering the comments above on the discrepancies between these two data sets, I also dispute the validity of the author’s analysis that the 5 sites they evaluated across the gyre accurately represent a cross section of the gyre. A lack of replication, low number of evaluated sites, and the apparent dispute of the author’s conclusions by the PP data set make it difficult to evaluate the validity of the author’s arguments.

I would suggest that the authors concentrate on the central theme of the paper, the novel deep penetration of O₂ into extremely oligotrophic sediments and the modeling of these profiles. The lack of replication and the lack of a complete data set make many of the conclusions of cross-gyre changes difficult to validate. Also a lack of specific station information (organic matter, porosities, grain sizes, microbial biomasses, etc.) makes conclusions across such a large spatial area very difficult.

Specific Comments:

Pg 3160 ln4: “most oligotrophic” – word choice is awkward

Addition of average C mineralization rates to abstract, as this is a significant result of this paper

Pg 3161 ln22: The use of the reference of D’Hondt 2009 throughout this MS is difficult to evaluate, as it is unpublished and un-reviewed. Please include data where appro-

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priate. It also makes it difficult to evaluate what data here is repetitious to the D'Hondt MS, however I realize this is always a challenge for the review process.

Ln 24: "subseafloor"

Ln26: "focusses"

Pg 3162 ln8: "metres"

Ln16: It is unclear as to the number of cores that are collected; I assume it is one at each station? While the reviewer appreciates the amount of work and resources that went into this MS, an n=1 makes comparisons across the cruise's large area difficult to validate.

Pg 3163 ln20: "In some cores" – please specify

Ln 18: I am very curious as to why only 5 of the cores were evaluated? Were the other cores evaluated and excluded? Please state in the text.

Pg 3164 ln3: remove comma after "both"

Ln 21: Why were the cores incubated at 20C, and not at 4C as for the Clark microsensor profiles (Pg 3163, ln 16)? This may cause an issue for comparing and combining both the measurements from the different methods, as well as the changes in O2 concentrations due to the warm incubation. Please explain and validate.

Pg 3165 - Modeling: I am curious as to why the authors chose to make their own model? A number of well-cited and robust models exist for the analysis for measured profiles (e.g. Berg et al. *Limnol. Oceanogr.* 1998)

Pg 3166 ln15: "exact"

Pg 3168 ln1: The authors state the exclusion of advection in transport processes due to the low permeability of sediments, but do not provide any data or reference to this fact. In contrast to this, the authors state that changes in porosity across sites may

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lead to an increase in fluxes (pg 3169 ln 15-18). As this research was conducted across such a large spatial scale I would expect some variation in porosity (and other factors). This issue needs to be addressed in the text and supported by data. Ln8: 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? I am particularly concerned about the in/ex situ comparisons of profiles, as the profile done in-situ produced a much larger uptake (station 10) compared to other profiles. How can sampling artifacts be exempted between these different conditions? In looking at the measured ex-situ profiles (with the exception of station 5), the profiles appear to be much more erratic and of a different shape than those of the very clean station 10.

Pg 3169 ln10-20: 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.

ln15-18: See comment above

Pg 3170 ln13: 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?

Ln 27: "was modeled exemplary" - I am not sure what this means

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Pg 3171 In2: "beeing"

Pg 3172 In14-15: Please provide data and/or references 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.

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