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Ocean Science Discussions

Interactive comment on "Subsurface primary production in the western subtropical North Pacific as evidence of large diapycnal diffusivity associated with the Subtropical Mode Water" *by* C. Sukigara et al.

C. Sukigara et al.

suki@pol.gp.tohoku.ac.jp

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To referee #2,

Thank you for your comment concerning the manuscript entitled "Subsurface primary production in the western subtropical North Pacific as evidence of large diapycnal diffusivity associated with the Subtropical Mode Water" which we submitted for publication in Ocean Science.

We have studied your comment very carefully. Regarding general comments, we agree that our description of the assumptions on which our argument was based on was not enough and that we should have interpreted the DO data more quantitatively. In revised manuscript, we will take completely different approach to argue the vertical large diffusivity based on more quantitative interpretation of the DO data. This approach is based on much fewer assumptions, which we will describe in detail in the revised manuscript.

Concerning the issue about an estimate of net primary production from the chlorophyll record by using data from the HOT, we agree that the argument depending on the HOT dataset is not strong enough. The only reason why we referred to the HOT record was that it was most replete with time-series physical and biogeochemical data; we do not have any further reasoning to apply the relationship derived from the HOT data to the western subtropical North Pacific. Therefore, we decided to take a completely difference approach to the argument as described below (see the reply to referee #1). We will delete the most of discussion of net community production (NCP) estimate based on the HOT dataset from the revised version to be submitted to Ocean Science and thus won't add any discussion about the relationship between the HOT and our dataset.

Concerning the problems arising from our a priori use of Kz of 5x10 -14 m2s-1 estimated by Qiu et al. combined with the nutrient profiles, we agree that our argument included some inconsistencies (too large f-ratio, etc.). Since our intention was to confirm the order of magnitude of the Kz (10-4 m2 s-1), we thought those were within the allowable range. However, your comment along with the comment from the other referees indicated that our original approach was confusing. We also agreed that more quantitative treatment of DO data would be useful. We thus decided to take a completely different approach by making more quantitative use of the float DO data as follows.

We won't assume the large diffusivity proposed by Qiu et al. a priori in the revised manuscript and rather will estimate vertical diffusivity by ourselves based on the oxygen

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profile time series. A key feature of the new approach is to estimate downward oxygen flux based on considerably smaller DO decrease rate in the layer immediately below the subsurface oxygen maximum (SOM) layer compared to that in the layer further below, which we interpret as the result of downward diffusive oxygen flux from the SOM layer. We then estimate the vertical diffusivity based on this vertical flux and the vertical gradient of DO concentration obtained by the float. Please see the reply to referee #1 for more details. We believe this new approach gives more direct evidence to support the large vertical diffusivity and resolves inconsistencies included in our previous arguments. We will also state more clearly that our intension is to present evidence for diffusivity of the order of 10-4 m2 s-1 and not to confirm a specific value of 5 * 10-4 m2 s-1.

Replies to other specific comments

(1) Concerning the range of nutrient supply, 10mgN m-2 d-1 was calculated by the production (78 mgN m-2 d-1) and the smallest f-ratio (0.1) as your comment, and 30 mgN was calculated by the large diffusivity (5 x 10-4 m2 s-1) and concentration gradient of nitrate. We will modify this description according to new estimation based on the new approach.

(2) This scaling factor is not a typical, because this observation in the subtropical gyre was one of the first attempts in the world. We decided the scaling factor (0.49) by the comparison between averages of the float observations and those of water samples. The goodness of fit of the calibration is not estimative. There is not the error estimate. Since we won't use chlorophyll data to estimate the production and only relative values of concentration will be of interest for the revised manuscript, we won't conduct such estimation.

(3) We calibrated the DO sensor during only the calibration cruise. We confirmed that the bias of DO didn't change with time significantly by checking DO values in the deep layer where DO concentration will not change. Kobayashi et al. (2006, "Neg-

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Discussion Paper

ative Bias of Dissolved Oxygen Measurements by Profiling Floats", Ocenography in Japan, Vol. 15, No.2, 479-498, in Japanese with English abstract and figure captions) examined nine profiling floats with oxygen sensors deployed by Tohoku University and JAMSTCE and demonstrated that the oxygen sensors show little or no degradation in performance.

(4) We will add the discussion about the spring bloom and the variation of chlorophyll.

(5) In the revise manuscript, we will use the revised Redfield ratio of Anderson and Sarmiento (1994).

(6) Temporal change in the diffusive flux of O2 will be mentined in the revised manuscript.

(7) Regarding O2/N ratio, our estimate as 9-20 was too rough to compare with previous studies. But, our new estimate mentioned in the reply to referee #1 fitted Redfield ratio.

(8) The chlorophyll maximum at the beginning of the plot at 75m might indicate the spring bloom, but that at the end of the plot is the one of the deep chlorophyll maximum. We will change the colorscale for clarity.

We will take into account all the technical issues raised. We found your comments most helpful. We hope revise manuscript will acceptable for publication.

Yours sincerely,

Chiho Sukigara Physical Oceanography Laboratory, Department of Geophysics, Graduate School of Science, Tohoku University 6-3, Aramaki-Aza-Aoba, Aoba-ku, Sendai, 980-8578, Japan. Tel: +81-22-795-6529, Fax: +81-22-795-6530, e-mail: suki@pol.gp.tohoku.ac.jp

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