

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.

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To referee #4,

Thank you for your comment concerning the above 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. We have to accept your criticism that we

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have no measurements of primary production and have only inferences of the quantity based on chlorophyll measurements and rudimentary bio-optical model. We now think our argument based on this indirect approach was rather weak, even though our intension was not to confirming the specific diffusivity value of $5 \times 10^{-4} \text{ m}^2 \text{ s}^{-1}$ but to present evidence for diffusivity of the order of $10^{-4} \text{ m}^2 \text{ s}^{-1}$.

Considering your comment along with the comment from the other referees, we decided to take a completely different approach with greater use of the time-series oxygen data of our float in the revised manuscript to be submitted to Ocean Science. 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 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 inconsistency (too large f-ratio, imbalance between nitrogen and oxygen cycles, etc.) included in our previous arguments. We will also state more clearly that our intension is to present evidence for diffusivity of the order of $10^{-4} \text{ m}^2 \text{ s}^{-1}$ and not to confirm a specific value of $5 \times 10^{-4} \text{ m}^2 \text{ s}^{-1}$.

We found your comments most helpful. We hope revise manuscript will acceptable for publication.

Yours sincerely,

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