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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.

Anonymous Referee #4

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The novel idea presented in this manuscript is the combination of the Qiu et al. (2006) diffusivity estimate with a nitrate profile (Figure 5) to estimate an upward flux of nitrate driven by turbulent processes. The value of 5 * 10-4 m2 s-1 used by Sukigara et al. is at the upper limit of the range reported by Qiu et al. (2-5 * 10-4 m2 s-1; see their Figure 10c), and is an order of magnitude larger than estimates of diffusivity based on microstructure measurements from that same area (Mori et al., 2008) and tracer release experiments elsewhere. The nature of the apparent discrepancy between the Qiu et al. estimate based on potential vorticity and the Mori et al. microstructure measurements

is not clear, despite Sukigara et al.'s dismissal of the Mori et al. measurements. Qiu et al. recognize that their estimate "should be considered an upper bound" because they attribute the observed seasonal erosion of STMW thickness solely to vertical diffusion, and other processes may also contribute.

In that sense, this paper constitutes more of a hypothesis than a conclusion. If the Qiu et al. diffusivity estimates are correct, then the biological consequences could be quite significant. What is really needed to test this idea is independent verification of the PV-based diffusivity estimate with additional microstructure and/or tracer measurements in the STMW region.

In this paper, Sukigara et al. suggest that subsurface primary production constitutes independent evidence for high diffusivity in STMW. Yet, Sukigara et al. have no measurements of primary productionâĂŤ only inferences of that quantity based on chlorophyll measurements and a rudimentary bio-optical model. These calculations suggest a nitrogen demand of 78 mg N m-2 d-1. Their estimate of upward diffusive supply of nitrate is 30 mg N m-2 d-1, which implies an f-ratio of 0.4. This f-ratio is four times higher than the f-ratio of 0.1 that one would expect for oligotrophic regions, so this does not constitute compelling evidence for high diffusivity in STMW.

Even more troubling is the apparent inconsistency between the new production inferred from the diffusivity estimate and the lack of oxygen accumulation in the 50-100m depth stratum. If nitrate were being supplied at a rate to sustain new production of 30 mg N m-2 d-1 over the 50-100m depth interval, a typical photosynthetic quotient would lead one would expect oxygen accumulation of ca. 0.8 ml/l over the 3 months of the time series. However, the oxygen concentration in the DCM shows no such increase.

The authors suggest that a downward diffusive flux of oxygen could account for the apparent discrepancy. However, the flux they estimate (30 mmol O2 m-2 d-1) is at least 50% larger than can be supported by the inferred new production (30 mg N m-2 d-1) * (1 mmol N / 14 mg N) * (106 mmol C / 16 mmol N) * (1.4 mmol O2 / 1 mmol

C) = 20 mmol O2 m-2 d-1. Therefore the analysis seems to suggest that the nitrogen and oxygen cycles are out of balance. However, their cryptic statements about the transport of nitrogen and oxygen seems to both confirm and deny this discrepancy. If the ratio varied between 9 and 20, how can it be characterized as close to the Redfield ratio of 8.6? I agree that 9 is close, but 20 is more than twice the canonical Redfield ratio. As written, the paper conveys an unclear impression about the degree to which the nitrogen and oxygen budgets can be closed.

Given all of these weaknesses, I cannot support publication of this paper in its present form.

Interactive comment on Ocean Sci. Discuss., 6, 1717, 2009.