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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.

Anonymous Referee #2

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GENERAL COMMENTS

In this paper, Sukigara and coauthors examine whether observed biological production is conisistent with recent estimates of large vertical diffusivity coefficients at the top of the subtropical mode water (STMW) zone. They do this by presenting five months of float data of chlorophyll, dissolved oxygen (DO), temperature, salinity, and potential vorticity as well data from CTD profiles collected on one validation cruise. Although the authors claim that their data confirms the recent estimate of Kz of 5x10-4, this claim is based on several assumptions that are not well documented in the paper.

Thus this paper should not be published in its present form. Additionally, some of the interpretation of the data, such as interpretation of the DO data, is qualitative, where a quantitative approach might have yielded more interesting results.

SPECIFIC COMMENTS

The central argument in this paper revolves around an estimate of net primary productivity (NPP) from a chlorophyll float record. The authors calculate NPP from the chlorophyll record by using data from the Hawaii Ocean Time-series (HOT) in order to derive a relationship between primary productivity and chlorophyll. One of the largest failings of this paper is that the authors do not describe how well this relationship works. This is especially worrisome since the relationship is likely to have problems because Chl:C ratios (and thus Chl:NPP) are known to be highly variable (as documented in many papers including Flynn, K.J., 2003. "Do we need complex mechanistic photoacclimation models for phytoplankton?" Limnology and Oceanography, 48(6): 2243-2249).

This paper would be greatly improved if it included a figure with the chlorophyll and NPP data from HOT, showing the proposed fit. Additionally, the authors should use statistical methods to describe how well their relationship fits the data. Furthermore, the authors state they average all the available HOT data from May 1998 to July 2007 first and then develop a relationship between the average chlorophyll profile and the average NPP profile. It might be possible to get a better fit if they make a climatology from the HOT NPP and chlorophyll data and then fit the climatological NPP with climatological chlorophyll since the Chl:C ratio may change with season because of changes in temperature, light penetration, etc.

Moreover, it is not clear whether a relationship derived for HOT is applicable to the western subtropical Pacific. The authors mention a difference in euphotic zone depth between HOT and their study site and that they try to correct for this by multiplying NPP estimate by the ratio in euphotic zone depths from their site and HOT. This is a good first step but perhaps a better correction could be made if they looked at the characteristics of the light penetration rather than simply euphotic zone depth. Also, the authors should expand their discussion to include other reasons (community structure, temperature, etc) why a fit for HOT may not be appropriate for their study site.

The second part of the argument in this paper is that the authors "confirm" a high estimate of Kz of 5x10-4 m2 s-1 by combining this Kz estimate with nitrate profiles and saying the amount of nitrate fluxed upward is consistent with NPP rate estimated from the chlorophyll data. The problem with this is that even if one accepts the primary production number from chlorophyll, it isn't clear how much nitrate is needed to support the production since a nitrate flux would be new nitrate and a lot of the production could be supported by regenerated nitrogen source.

The authors do address this question but not clearly enough. Using a Kz of 5x10-4 m2 s-1, the authors calculate a diffusive nitrate supply of 30 mg N m-2 d-1. From the chl:NPP fit, they predict NPP is 78 mg N m-2 d-1. The f-ratio (ratio of new to total production) is therefore 0.38 which is higher than the common estimates of f-ratios for subtropical gyres of 0.1 to 0.25. This difference in f-ratio suggests that Kz should be smaller than 5x10-4 m2 s-1 or that there is a problem with the NPP estimates. Hence I think the central conclusion of their paper – that large Kz are consistent with the data – is questionable. If this paper is just trying to determine the order of magnitude of the Kz value – i.e. it is on the order of 1x10-4 rather than 1x10-5, then this f-ratio argument is OK. But as stated, the authors seem to be confirming a specific value of 5x10-4 m2 s-1, and that level of certainty can't be supported.

In the paper, the authors do not calculate the f-ratio for their data directly. Instead they use an estimate of f-ratio = 0.25 to predict a diffusive nitrate supply of 19.5 mg N m-2 d-1 and say that this nitrate supply is consistent with the 30 mg N m-2 d-1 that they calculate based on the nitrate profile and a Kz of 5x10-4 m2 s-1 . The problem is that 30 mg N m-2 d-1 is above the upper limit given by assuming an f-ratio of 0.25. Additionally, there is no evidence that all the required new nitrate comes from mixing rather than from nitrogen fixation, lateral advection of DON, etc. Thus a smaller Kz

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might be warranted.

The authors base the diffusivity argument on profiles of nitrate taken during a cruise that was used to calibrate the sensors. Additionally, they use the Chl:NPP relationship to estimate NPP. However, they do not do much quantitative with the rest of their float data. In particular, they only do qualitative interpretation of the dissolved O2 data. I realize it is hard to estimate production from only O2 data since O2 responds to biology and physical factors such as gas exchange and thermal forcing. Thus O2 is often paired with Ar, an inert gas, in order to untangle the physical from biological signal. There is no way to make Ar measurements on floats and thus O2 data is all they have. Still, it would be nice to have seen a quantitative treatment of the O2 data, perhaps using a simple model to estimate the sizes of gas exchange, thermal heating, mixing, production, similar to as had been done in Mourino-Carballido and Anderson, 2009 (Mourino-Carballido, B. and Anderson, L.A., 2009. "Net community production of oxygen derived from in vitro and in situ 1-D modeling techniques in a cyclonic mesoscale eddy in the Sargasso Sea." Biogeosciences, 6: 1799-1810.).

Other more specific scientific comments I have are listed below.

1) p. 1718: In abstract: sentence that "vertically integrated chlorophyll values during this period consistently ranged from 15-30 mg N m-2, indicating sustained primary production and a continuous supply of nutrients ranging from 10 to 30 mg N m-2 day 1." How do the authors calculate the 10 to 30 mg N m-2 d-1 number? If production is 78 mg N m-2 d-1, and an f-ratio of 0.1 to 0.25 is assumed, then nutrient supply should be 8 to 20 mg N m-2 d-1.

2) p. 1721: "data from chlorophyll sensor was scaled by 0.49". Is this a typical scaling factor for such sensors? What was the goodness of fit of the calibrations? Is there an error estimate for this scaling factor?

3) p. 1721: "DO sensor of float had a bias of -0.15 ml L-1". Was the DO sensor only calibrated during the calibration cruise or was it also calibrated at the beginning and

OSD

6, C517–C522, 2009

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end of deployment? Such sensors are known to have problems with biofouling. If the sensor was only calibrated during the cruise, why is it assumed that the bias was constant with time? Was biofouling investigated?

4) p. 1721: it is surprising to me that you can have a spring bloom while the mixed layer depth is still 300 m, as is stated in this paper. The surface chlorophyll seems highest when the mixed layer is shallower. More discussion on how/why they defined the spring bloom would be useful. 5) p. 1723: Why were the Redf ield ratios used rather than the revised Redfield ratios of Anderson and Sarmiento, 1994 (Anderson, L.A. and Sarmiento, J.L., 1994. Redfield ratios of remineralization determined by nutrient data-analysis. Global Biogeochemical Cycles, 8(1): 65-80.)?

6) p. 1724: It would be interesting to see a plot of how the diffusive flux of O2 changed with time.

7) p. 1724: The authors calculate the ratio of nitrate to oxygen transport as 9 to 20 and state that it was similar to Redfield ratio of 8.6 but not exactly the same perhaps because nitrate profile was only in June whereas O2 data was between May and July. I have two comments here. One, it would be better to compare to the revised Redfield ratio of Anderson and Sarmiento, which yield a ratio of O2:N of 10.6 rather than 8.6. Secondly, and more importantly, it is not surprising that the O2/N ratio does not match the Redfield ratio at all times since variable C/N ratios (and thus O2/N ratios by extension) have often been observed – see paper by Ono et al, 2001 for example. (Ono, S., Ennyu, A., Najjar, R.G. and Bates, N.R., 2001. "Shallow remineralization in the Sargasso Sea estimated from seasonal variations in oxygen, dissolved inorganic carbon and nitrate." Deep-Sea Research, 48: 1567-1582.)

8) Fig. 3c: There are maxima in the chlorophyll distributions at the beginning and end of the plot at about 75 m depth. Are these signs of the spring/fall blooms? Or are these artifacts? Also, in Fig. 3c, the colorscale makes it difficult to see the variations in Chl. Consider changing to a colorscale similar to the other plots instead of just using green.

OSD

6, C517–C522, 2009

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TECHNICAL ISSUES

1) p. 1719: “PV” should be defined as potential vorticity the first time it is mentioned in the text (rather than only being defined in the figure caption).

2) p. 1722: “satulation” should be “saturation.”

3) p. 1724: “Note that the production advanced by this upward nutrient supply from the STMW layer had been not reflected in the DO time-series variation as net DO increment from May to July.” This should be “was not reflected"

4) p. 1724: “Do concentration in the subsurface oxygen maximum was not increase regardless of. . .”. Should be “did not increase”

5) p. 1726: “which also gives large impact” should be “which also largely impacts”

6) Throughout the paper: O2 is reported in units of ml L-1 but the standard unit is umol kg-1.

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

OSD

6, C517–C522, 2009

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